PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LXIII. PART I.

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M.DCC.LXXIII.

Jos: Banks

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume. And this information was thought the more neceſſary, not only as it had been the common opinion, that they were publiſhed by the authority, and under the direction, of the Society itſelf; but alſo, becauſe ſeveral authors, both at home and abroad, have in their writings called them the *Tranſactions of the Royal Society*. Whereas in truth the Society, as a body, never did intereſt themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the

reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

At a COUNCIL, January 28, 1773.

Reſolved, That after Volume LXII. the *Philoſophical Tranſactions* be publiſhed twice in a year; the firſt publication to be of the months of November and December of the preceding year, and January and February of the current year, as ſoon as may be after February, under the name of the "firſt part" of the volume: and the ſecond publication to be of the remaining months unto the receſs of the Society, as ſoon as may be after the receſs, under the name of the "ſecond part" of the volume.

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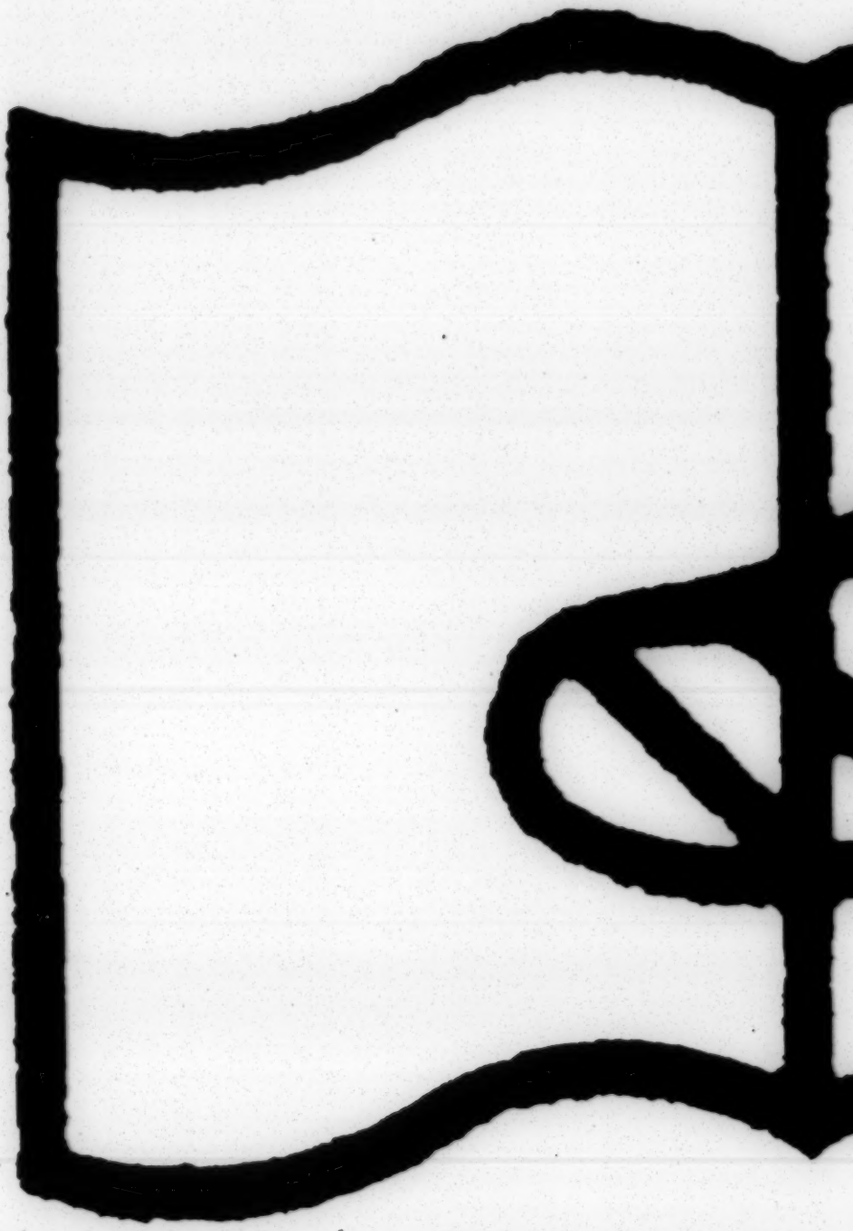
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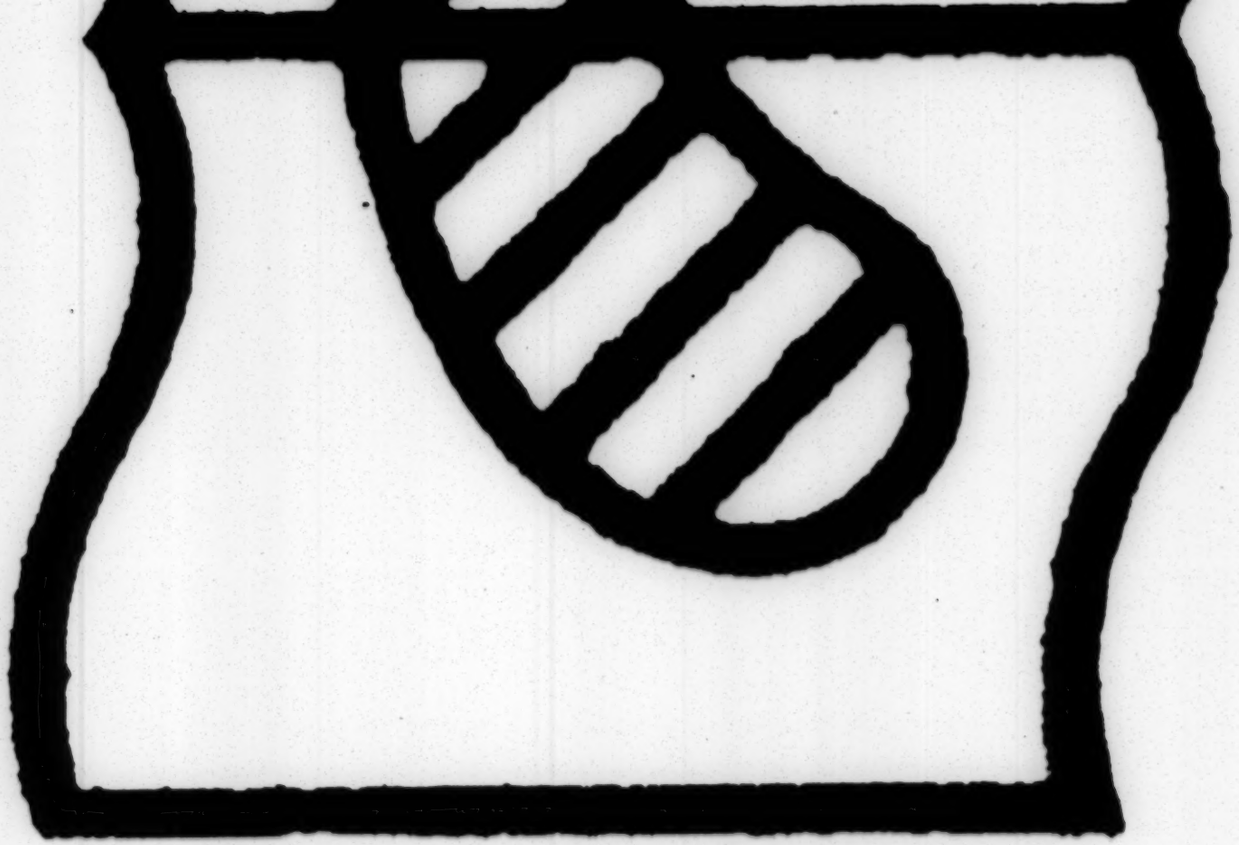
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Matthew Stewart, D.D. { *Pr. Mat. Edin.*
James Stuart, Eſq;
Philip Van Swinden, M. A.
Sidney Swinney, D. D.
John Swinton, B. D.
Edward Thomas, A. M.
Noah Thomas, M.D. { *Med. R. Ext.*
Sam. Welles Thomſon, LL. D.
JOHN Earl of TILNEY.
Anthony Tiffington, Eſq;
ROBERT Lord TREVOR.
George Forſter Tuffnell, Eſq;
Marmaduke Tunſtal, Eſq;
John Turton, M.D. { *Regin. ad Fam.*
Thomas Tyrwhitt, Eſq;
Rodolph Vall Travers, Eſq;
RALPH Earl VERNEY.
John Upton, Eſq;
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Richard Hill Waring, Eſq;
Mr. Joſeph Warner.
Richard Warren, M.D. *Med. Reg.*
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Jonathan Watſon, Eſq;
Richard Watſon, D.D. { *Th. R. Pr. Cant.*
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Benjamin Way, Eſq;
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William Webber, Eſq;
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Thomas Wilbraham, M. D.
John Wilkes, Eſq;
Iſrael Wilkes, Eſq;
John Wilkinſon, M.D. { *S. Gott. Soc.*
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John Winthrop, Eſq;
Sir Clifton Wintringham, Knt. M.D. [*Med. Reg.*
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Francis Wollaſton, Eſq;
George Wollaſton, A. M.
Thomas Wood, Eſq;
Mr. Peter Woulfe.
Daniel Wray, Eſq; *Muſ. Brit. Car.*
Richard Wright, M. D.
William Wyatt, M. A.
Mr. Thomas Yeoman.
Hon. John Yorke.
Sir William Younge, Bart.

*** *Ten of the* Fellows *of the* Society *are to be choſen into the* Council *for the Year enſuing, on the* 30th *of* November 1774, *being* SAINT ANDREW'S DAY.

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lbert de Haller, *M. D. Reg. Ac. Sc. Par. S. & Pr. S. Gott.*
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Ds. Franc. Maria Zanotti, *Sec. Ac. Sc. Inſt. Bonon.*

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Diſcovery of the Manner of making Iſinglaſs in* Ruſſia; *with a particular Deſcription of its Manufacture in* England, *from the Produce of* Britiſh *Fiſheries. In a Letter from* Humphrey Jackſon, *Eſq*; *F. R. S. to* William Watſon, *M. D. F. R. S.*

SIR,

Read Nov. 19, 1772. YOUR diſtinguiſhed zeal to promote ſcience in general, and particularly ſuch arts as tend to improve the commerce

of your country, incites me to addreſs to you the following account of the genuine manufacture of foreign iſinglaſs, with the method of making it in England from Britiſh materials. If you ſhould judge the ſubject deſerving attention, you will do me great honour in preſenting this paper to the Royal Society: but this I beg leave to ſubmit to your judgement and candour, and to ſubſcribe myſelf,

With great eſteem,

SIR,

Your moſt obliged,

and moſt obedient

humble ſervant,

Great Tower-Hill,
June 15, 1772.

Humphrey Jackſon.

ALL

ALL authors, who have hitherto delivered proceſſes for making icthyocolla, fiſh-glue or iſinglaſs, have greatly miſtaken both its conſtituent matter and preparation.

To prove this aſſertion, it may not be improper to recite what Pomet ſays upon the ſubject, as he appears to be the principal author whom the reſt have copied *. After deſcribing the fiſh, and referring to a cut engraved from an original in his cuſtody, he ſays: 'As to the manner of making the 'iſinglaſs, the ſinewy parts of the fiſh are boiled 'in water, till all of them be diſſolved that will diſ-'ſolve; then the gluey liquor is ſtrained, and ſet to 'cool. Being cold, the fat is carefully taken off, 'and the liquor itſelf boiled to a juſt conſiſtency, 'then cut to pieces, and made into a twiſt, bent in 'form of a creſcent, as commonly ſold, then hung 'upon a ſtring, and carefully dried.'

From this account, it might be rationally concluded that every ſpecies of fiſh which contained gelatinous principles would yield iſinglaſs: and this parity of reaſoning ſeems to have given riſe to the haſty concluſions of thoſe, who ſtrenuouſly vouch for the extraction of iſinglaſs from ſturgeon; but as that fiſh is eaſily procureable, the negligence of aſcertaining the fact by experiment ſeems inexcuſeable.

Every traveller, as well as author, who mentions iſinglaſs, obſerves that it is made from certain fiſh

* See Pomet's Hiſtory of Drugs, and Caſpar Neuman's Chemiſtry, Engliſh tranſlations. Hiſt. Materiæ Medicæ, Vogel. Lewis's Materia Medica. Doſſie's Inſtitutes of Chemiſtry.

found in the Danube and rivers of Muſcovy. Willughby and others inform us, that it is made of the ſound of the * Beluga; Caſpar Newman that it is made of the Huſo Germanorum and other fiſh, which he has ſeen frequently ſold in the public markets of Vienna. Theſe circumſtances make it appear the more extraordinary, that a perfect account of the manufacture of ſuch an eſſential article of commerce ſhould remain ſo long unrevealed.

In my firſt attempts to diſcover the conſtituent parts and manufacture of iſinglaſs, relying too much upon the authority of ſome chemical authors, whoſe veracity I had experienced in many other inſtances, I found myſelf conſtantly diſappointed. Glue, not iſinglaſs, was the reſult of every proceſs; and although, in the ſame view, a journey to Ruſſia proved fruitleſs, yet a ſteady perſeverance in the reſearch proved not only ſucceſsful as to this object, but, in the purſuit to diſcover a reſinous matter plentifully procureable in the † Britiſh fiſheries, which has been found, by ample experience, to anſwer ſimilar purpoſes. It is now no longer a ſecret that our ‡ lakes and rivers in North America are ſtocked

* Vide Specimen Hiſtor. Nat. Volg. Auctore J. R. Forſter, Philoſ. Tranſ. 1767.

† Upwards of forty tons of Britiſh iſinglaſs have been manufactured and conſumed ſince this diſcovery was firſt made.

‡ As the lakes of North America lie nearly in the ſame latitude with the Caſpian Sea, particularly Lake Superior, which is ſaid to be of greater extent, it was conjectured they might abound with the ſame ſorts of fiſh, and, in conſequence of public advertiſements diſtributed in various parts of North America, offering premiums for the ſounds of ſturgeon, and other fiſh, for the purpoſe of making iſinglaſs, ſeveral ſpecimens of fine

with

with immenſe quantities of fiſh, ſaid to be the ſame ſpecies with thoſe in Muſcovy, and yielding the fineſt iſinglaſs, the fiſheries whereof, under due encouragement, would doubtleſs ſupply all Europe with this valuable article.

But to return, no artificial heat is neceſſary to the production of iſinglaſs, neither is the matter diſſolved for this purpoſe; for, as the continuity of its fibres would be deſtroyed by ſolution, the maſs would become brittle in drying, and ſnap ſhort aſunder, which is always the caſe with glue, but never with iſinglaſs. The latter, indeed, may be reſolved into glue with boiling water, but its fibrous recompoſition would be found impracticable afterwards, and a fibrous texture is one of the moſt diſtinguiſhing characteriſtics of genuine iſinglaſs. The reproduction of leather might, with equal reaſon, be attempted from the former.

A due conſideration that an imperfect ſolution of iſinglaſs, called fining by the brewers, poſſeſſed a peculiar property of clarifying malt liquors, induced me to attempt its analyſis in cold ſubacid menſtruums. One ounce and a half of good iſinglaſs, ſteeped a few days in one gallon of ſtale beer, was converted into good fining, of a remarkable thick conſiſtence: the ſame quantity of glue, under ſimilar treatment, yielded only a mucilaginous liquor, reſembling diluted gum-water, which, inſtead of clarifying beer, increaſed both its tenacity and turbidneſs, and communicated other properties in no

iſinglaſs, the produce of fiſh taken in theſe parts, have been lately ſent to England, with proper atteſtations as to the unlimited quantity which may be procured.

reſpect

reſpect correſponding with thoſe of genuine fining. On commixing three ſpoonfuls with a gallon of malt liquor, in a tall cylindrical glaſs, a vaſt number of curdly maſſes became preſently formed, by the reciprocal attraction of the particles of iſinglaſs and the feculencies of the beer, which, increaſing in magnitude and ſpecific gravity, arranged themſelves accordingly, and fell in a combined ſtate to the bottom, through the well-known laws of gravitation; for, in this caſe, there is no elective attraction, as ſome have imagined, which bears the leaſt affinity with what frequently occurs in chemical decompoſitions.

Theſe phænomena are adduced here as correlative proofs of the impracticability of making iſinglaſs by the previous reduction of the ſinewy parts of fiſh into jelly; and it ſeems evident, that the clarifying action of iſinglaſs depends principally upon a crude minute diviſion, not ſolution of its parts, which is ſtill farther confirmed, by diluting a few drops of fining with fair water in a glaſs; for thus the ſlender filaments become conſpicuous to the eye, eſpecially when aſſiſted with a double convex lens, but theſe immediately diſappear on an addition of hot water.

As the general proceſſes for making iſinglaſs appear from hence illuſive and erroneous, the long concealed principles of its manufacture into the various common forms and ſhapes become more obvious and comprehenſive. If what is commercially termed long or ſhort ſtapled iſinglaſs be ſteeped a few hours in fair cold water, the entwiſted membranes will expand, and reaſſume their original beau-

ful * hue, and, by a dextrous addreſs, may be perfectly unfolded. By this ſimple operation, we find that iſinglaſs is actually nothing more than certain membranous parts of fiſhes, diveſted of their native mucoſity, rolled and twiſted into the forms above-mentioned, and dried in the open air.

The ſounds, or air-bladders of freſh-water fiſh, in general, are preferred for this purpoſe, as being the moſt tranſparent, flexible, delicate ſubſtances. Theſe conſtitute the fineſt ſorts of iſinglaſs; thoſe called book and ordinary ſtaple, are made of the inteſtines, and probably the peritonæum, of the fiſh. The Beluga yields the greateſt quantity, as being the largeſt and moſt plentiful fiſh in the Muſcovy rivers; but the ſounds of all freſh-water fiſh yield, more or leſs, fine iſinglaſs, particularly the ſmaller ſorts, found in prodigious quantities in the Caſpian ſea, and ſeveral hundred miles beyond Aſtracan, in the Wolga, Yaik, Don, and even as far as Siberia, where it is called *kle* or *kla* by the natives, which implies a glutinous matter; it is the baſis of the Ruſſian glue, which is preferred to all other kinds for its ſtrength.

The anatomy and † uſes of the ſound in fiſh ſeems not yet adjuſted by ichthyologiſts. I have not met with a genuine deſcription of its ſituation and

* If the fine tranſparent iſinglaſs be held in certain poſitions to the light, it frequently exhibits beautiful priſmatic colours.

† Fiſhermen have a dextrous art in perforating the ſound of freſh-taken cod fiſh with a needle, in order to diſengage the incloſed air. Without this operation, the fiſh could not be kept under water in the well-boat, conſequently could not live; but if by accident the operator wounds an artery, the fiſh preſently dies, through the diſcharge of bood, to the loſs of the proprietor, who thus can ſeldom bring it ſweet to market.

figure

figure in any author. A modern writer * will have it to be the meſentery of the fiſh; but the celebrated Gouan, the lateſt, and perhaps the moſt accurate author on icthyology, gives a more ſatisfactory and comprehenſive account of it, under the title of † *La Veſicule Aërienne.* Yet, if the identity of the air-bladder, and what, in Engliſh, is called ſound, be admitted, which ſeems particularly aſcertained in a certain genus, viz. the Aſellus of Willughby, or Gadus of Artedi, his deſcription is a little erroneous with reſpect to its termination near the *Veſica urinaria*; for in cod and ling, the continuation of the ſound, or air-bladder, may be eaſily traced from thence to the laſt *vertebra* adjoining the tail.

The ſounds, which yield the finer iſinglaſs, conſiſt of parallel fibres, and are eaſily rent longitudinally; but the ordinary ſorts are found compoſod of double membranes, whoſe fibres croſs each other obliquely, reſembling the coats of a bladder; hence the former are more readily pervaded and divided with ſubacid

* Doſſie, in Memoirs of Agriculture.

† La Véſicule aërienne eſt un ſac membraneux compoſé de deux ou trois envelopes, qui ſe ſeparent facilement, & rempli d'air, à la faveur duquel les poiſſons ſe ſoutiennent dans l'eau. Il eſt pour l'ordinaire ſitué en long, enfermé dans le peritoine, placé entre les vertebres & l'eſtomac. Sa longueur dépend de la capacité du bas ventre, & de la grandeur du poiſſon: il eſt tantôt cylindrique, elliptique, ové ou renverſé, tantôt à deux lobes & à deux loges, tantôt à trois lobes & à trois loges, &c. dans les males il deſcend preſque juſqu' à la region de la veſſie urinaire.

Cette Veſicule eſt attachée avec l'eſtomac, avec l'eſophage, ſans le diaphragme, tantôt par le côté tantôt par la pointe & ſ'y abbouche par un conduit pneumatique. Goüan, Hiſtoire des Poiſſons.

liquors;

liquors; but the latter, through a peculiar kind of interwoven texture, are with great difficulty torn aſunder, and long reſiſt the power of the ſame menſtruum; yet, when duly reſolved, are found to act with equal energy in clarifying liquors.

Iſinglaſs receives its different ſhapes in the following manner.

The parts, of which it is compoſed, particularly the ſounds, are taken from the fiſh while ſweet and freſh, ſlit open, waſhed from their ſlimy *ſordes*, diveſted of every thin membrane which invelopes the ſound, and then expoſed to ſtiffen a little in the air. In this ſtate, they are formed into rolls about the thickneſs of a finger, and in length according to the intended ſize of the ſtaple: a thin membrane is generally ſelected for the center of the roll, round which the reſt are folded alternately, and about half an inch of each extremity of the roll is turned inwards. The due dimenſions being thus obtained, the two ends of what is called ſhort ſtaple are pinned together with a ſmall wooden * peg; the middle of the roll is then preſſed a little downwards, which gives it the reſemblance of a heart ſhape, and thus it is laid on boards, or hung up in the air to dry. The ſounds, which compoſe the long ſtaple, are larger than the former; but the operator lengthens this ſort at pleaſure, by interfolding the ends of one or more pieces of the ſound with each other. The extremities are faſtened with a peg, like the former; but the middle part of the roll is bent more conſiderably down-

* See the annexed Drawings [Tab. I. Fig. 1.]

wards; and, in order to preſerve the * ſhape of the three obtuſe angles thus formed, a piece of round ſtick, about a quarter of an inch diameter, is faſtened in each angle with ſmall wooden pegs, in the ſame manner as the ends. In this ſtate, it is permitted to dry long enough to retain its form, when the pegs and ſticks are taken out, and the drying completed; laſtly, the pieces of iſinglaſs are colligated in rows, by running packthread through the peg-holes, for convenience of package and exportation.

The membranes of the † book ſort, being thick and refractory, will not admit a ſimilar formation with the preceding: the pieces therefore, after their ſides are folded inwardly, are bent in the center, in ſuch manner that the oppoſite ſides reſemble the cover of a book, from whence its name; a peg being run acroſs the middle, faſtens the ſides together, and thus it is dried like the former. This ſort is interleaved, and the pegs run acroſs the ends, the better to prevent its unfolding.

That called cake iſinglaſs is formed of the bits and fragments of the ſtaple ſorts, put into a flat metalline pan, with a very little water, and heated juſt enough to make the parts cohere like a pancake, when it is dried; but frequently it is overheated, and ſuch pieces, as before obſerved, are uſeleſs in the buſineſs of fining. Experience has taught the conſumers to reject them.

Iſinglaſs is beſt made in the ſummer, as froſt gives it a diſagreeable colour, deprives it of weight, and

* See Fig. 3.
† Fig. 4.

impairs

impairs its gelatinous principles; its faſhionable forms are unneceſſary, and frequently injurious to its native qualities. It is common to find oily putrid matter and *exuviæ* of inſects between the implicated membranes, which, through the inattention of the cellarman, often contaminate wines and malt liquors in the act of clarification. Theſe peculiar ſhapes might, probably, be introduced originally with a view to conceal and diſguiſe the real ſubſtance of iſinglaſs, and preſerve the monopoly; but, as the maſk is now taken off, it cannot be doubted to anſwer every purpoſe more effectually in its * native ſtate, without any ſubſequent manufacture whatever, eſpecially to the principal conſumers, who hence will be enabled to procure ſufficient ſupply from the Britiſh colonies. Until this laudable end can be fully accompliſhed, and as a ſpecies of iſinglaſs, more eaſily producible from the marine fiſheries, may probably be more immediately encouraged, it may be manufactured as follows.

The ſounds of cod and ling bear great analogy with thoſe of the *accipenſer genus* of Linnæus and Artedi, and are in general ſo well known, as to require no particular deſcription. The Newfoundland and Iceland fiſhermen ſplit open the fiſh, as ſoon as taken, and throw the back-bones, with the ſounds annexed, in a heap; but, previous to incipient putrefaction, the ſounds are cut out, waſhed from their ſlimes, and ſalted for uſe. In cutting out the ſounds, the intercoſtal parts are left behind, which are much the beſt; the Iceland fiſhermen are ſo ſenſible of

* See Fig. 5.

this, that they beat the bone upon a block with a thick ſtick, till the pockets, as they term them, come out eaſily, and thus preſerve the ſound entire. If the ſounds have been cured with ſalt, that muſt be diſſolved by ſteeping them in water, before they are prepared for iſinglaſs; the freſh ſound muſt then be laid upon a block of wood, whoſe ſurface is a little elliptical, to the end of which a ſmall hair bruſh is nailed, and with a * ſaw-knife, the membranes on each ſide of the ſound muſt be ſcraped off. The knife is rubbed upon the bruſh occaſionally, to clear its teeth; the pockets are cut open with ſciſſars, and perfectly cleanſed of the mucous matter with a coarſe cloth: the ſounds are afterwards waſhed a few minutes in lime-water, in order to abſorb their oily principle, and laſtly in clear water. They are then laid upon nets, to dry in the air; but, if intended to reſemble foreign iſinglaſs, the ſounds of cod will only admit of that called book, but thoſe of ling both ſhapes. The thicker the ſounds are, the better the iſinglaſs, colour excepted; but that is immaterial to the brewer, who is its chief conſumer.

This iſinglaſs reſolves into fining, like the other ſorts, in ſubacid liquors, as ſtale beer, cyder, old hock, &c. and in equal quantities produces ſimilar effects upon turbid liquors, except that it falls ſpeedier and cloſer to the bottom of the veſſel, as may be demonſtrated in tall cylindrical glaſſes; but foreign iſinglaſs retains the conſiſtency of fining preferably in warm weather, owing to the greater tenacity of its native mucilage.

* See Fig. 2.

Vegetable

Vegetable acids are, in every reſpect, beſt adapted to fining: the mineral acids are too corroſive, and even inſalubrious in common beverage.

It is remarkable that, during the converſion of iſinglaſs into fining, the acidity of the menſtruum ſeems greatly diminiſhed, at leaſt to taſte, not on account of any alkaline property in the iſinglaſs, probably, but by its inveloping the acid particles. It is likewiſe reducible into jelly with alkaline liquors, which indeed are ſolvents of all animal matters; even cold lime-water diſſolves it into a pulpous *magma*. Notwithſtanding this is inadmiſſible as fining, on account of the menſtruum, it produces an admirable effect in other reſpects: for, on commixture with compoſitions of plaſter, lime, &c. for ornamenting walls expoſed to viciſſitudes of weather, it adds firmneſs and permanency to the cement; and if common brick-mortar be worked up with this jelly, it ſoon becomes almoſt as hard as the brick itſelf: but, for this purpoſe, it is more commodiouſly prepared, by diſſolving it in cold water, acidulated with vitriolic acid; in which caſe, the acid quits the jelly, and forms with the lime a *ſelenitic* maſs, while, at the ſame time, the jelly being deprived, in ſome meaſure, of its moiſture, through the formation of an indiſſoluble concrete amongſt its parts, ſoon dries, and hardens into a firm body; whence its ſuperior ſtrength and durability are eaſily comprehended.

It has long been a prevalent opinion, that ſturgeon, on account of its cartilaginous nature, would yield great quantities of iſinglaſs; but, on examination, no part of this fiſh, except the inner coat of the ſound, promiſed the leaſt ſucceſs. This being full

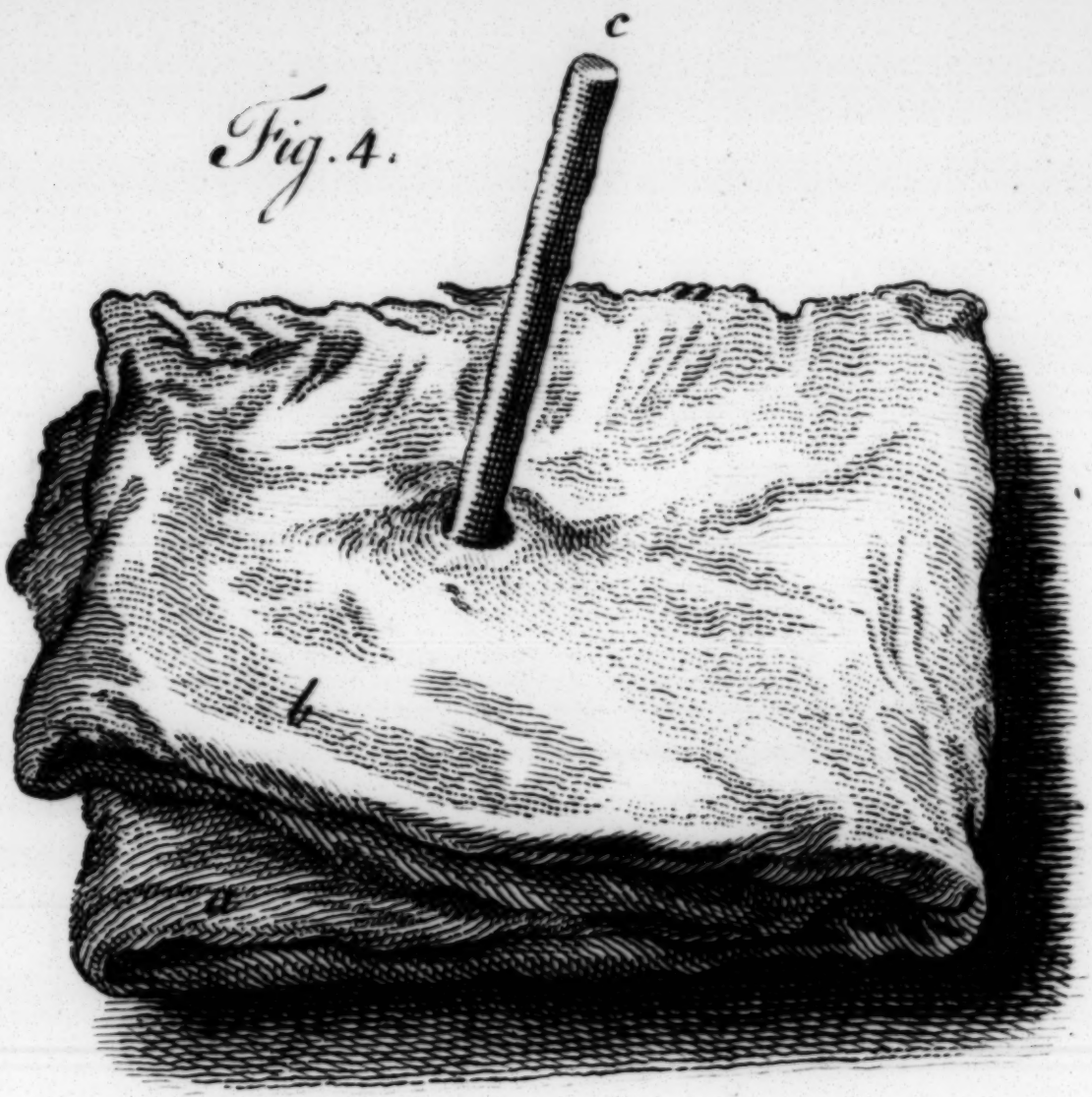
of

of *rugæ*, adheres ſo firmly to the external membrane, which is uſeleſs, that the labour of ſeparating them ſuperſedes the advantage. The inteſtines, however, which in the larger fiſh extend ſeveral yards in length, being cleanſed from their mucus, and dried, were found ſurprizingly ſtrong and elaſtic, reſembling cords made with the inteſtines of other animals, commonly called cat-gut, and, from ſome trials, promiſed ſuperior advantages, when applied to mechanic operations.

Having now ſufficiently revealed the principal *arcana* in the manufacture of iſinglaſs, and explained ſome of its leaſt known phænomena and properties, the farther proſecution thereof, as a commercial buſineſs, is left to others, whoſe future inquiries into the ſubject, it is hoped, will, in ſome reſpect, be anticipated through this narrative; but whatever ſucceſs may attend the attempt, I flatter myſelf to ſtand acquitted, in having contributed every thing in my power to its advancement and perfection.

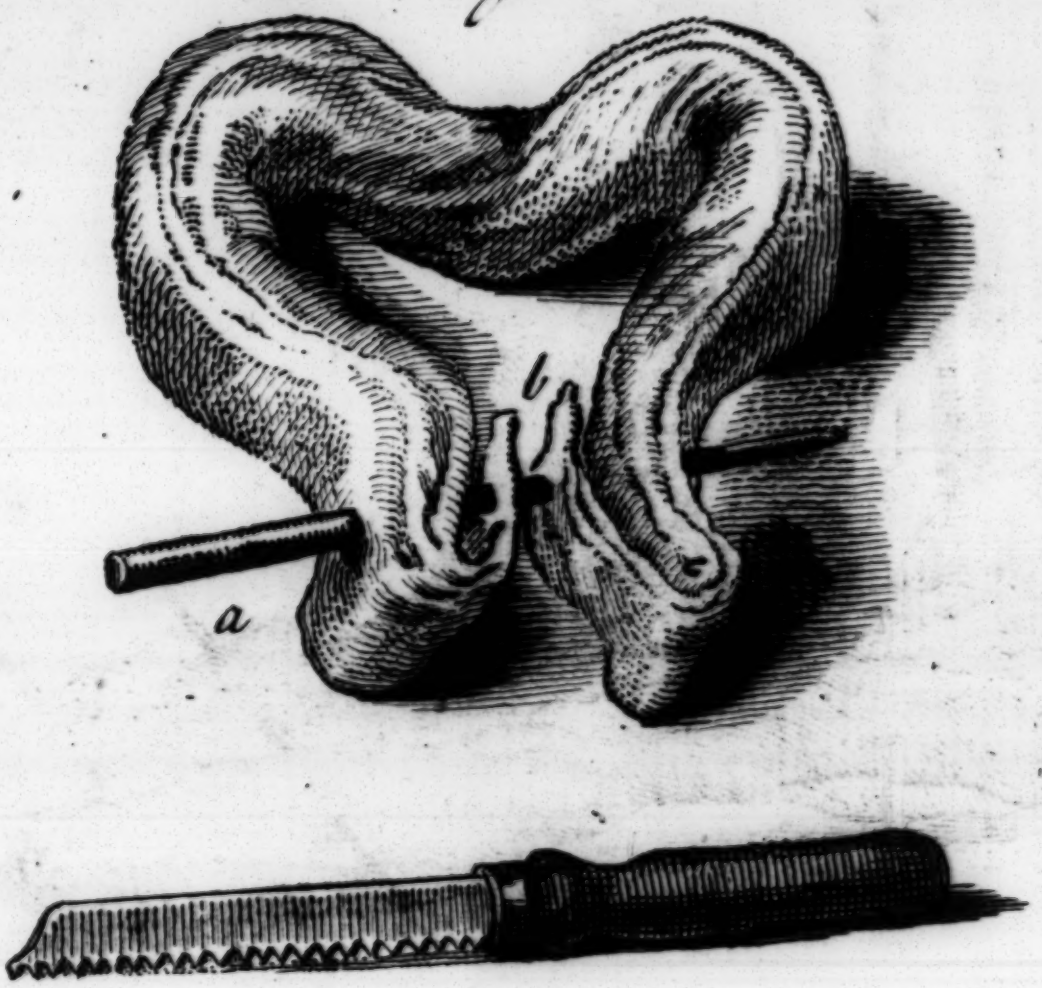
EXPLANATION

Fig. 4.



Basire Sc.

Fig. 1



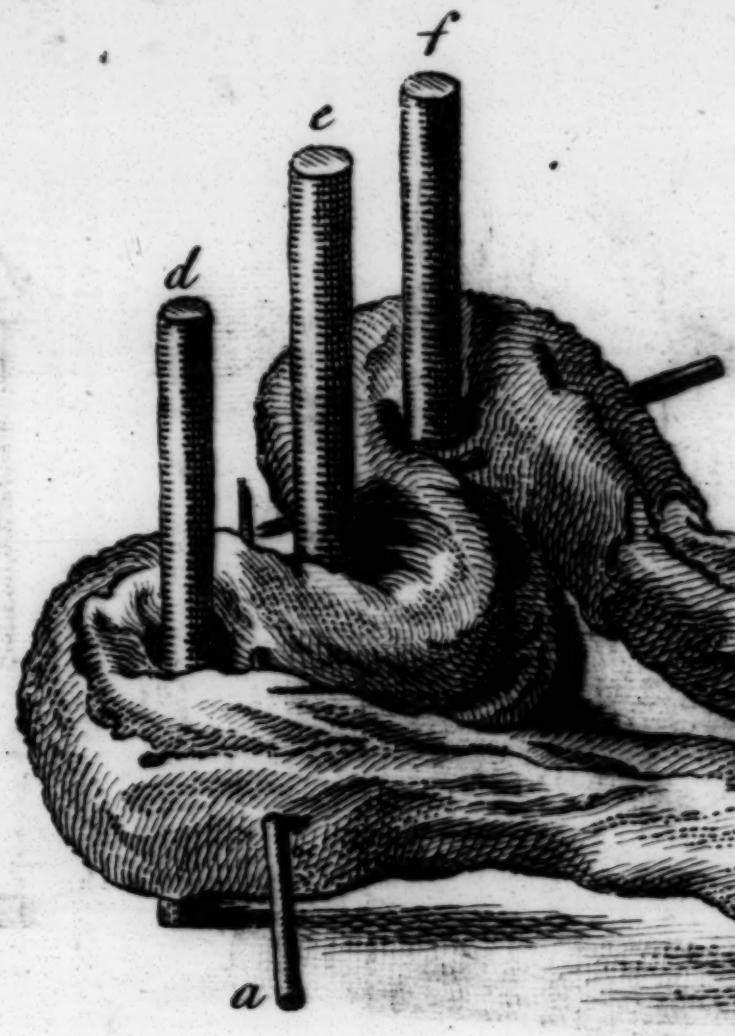


Fig. 2.

Fig

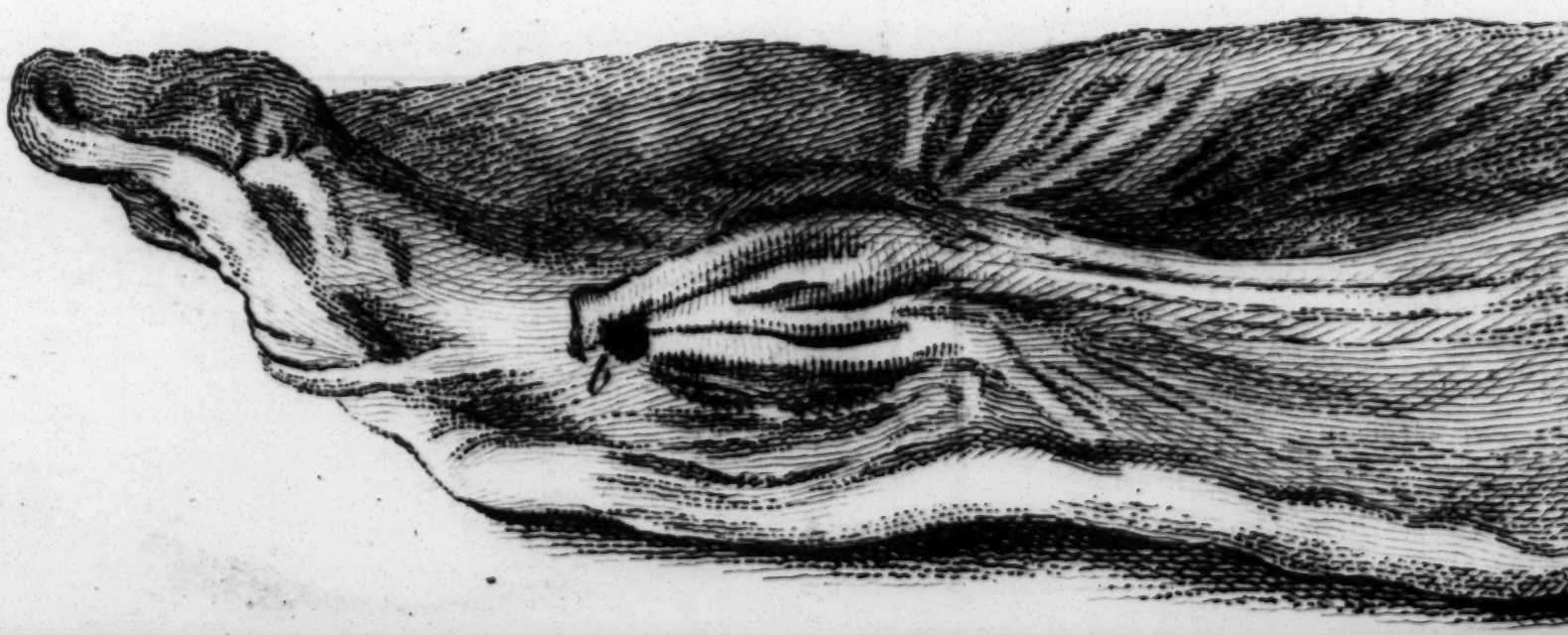


Fig. 3.



Fig. 4.

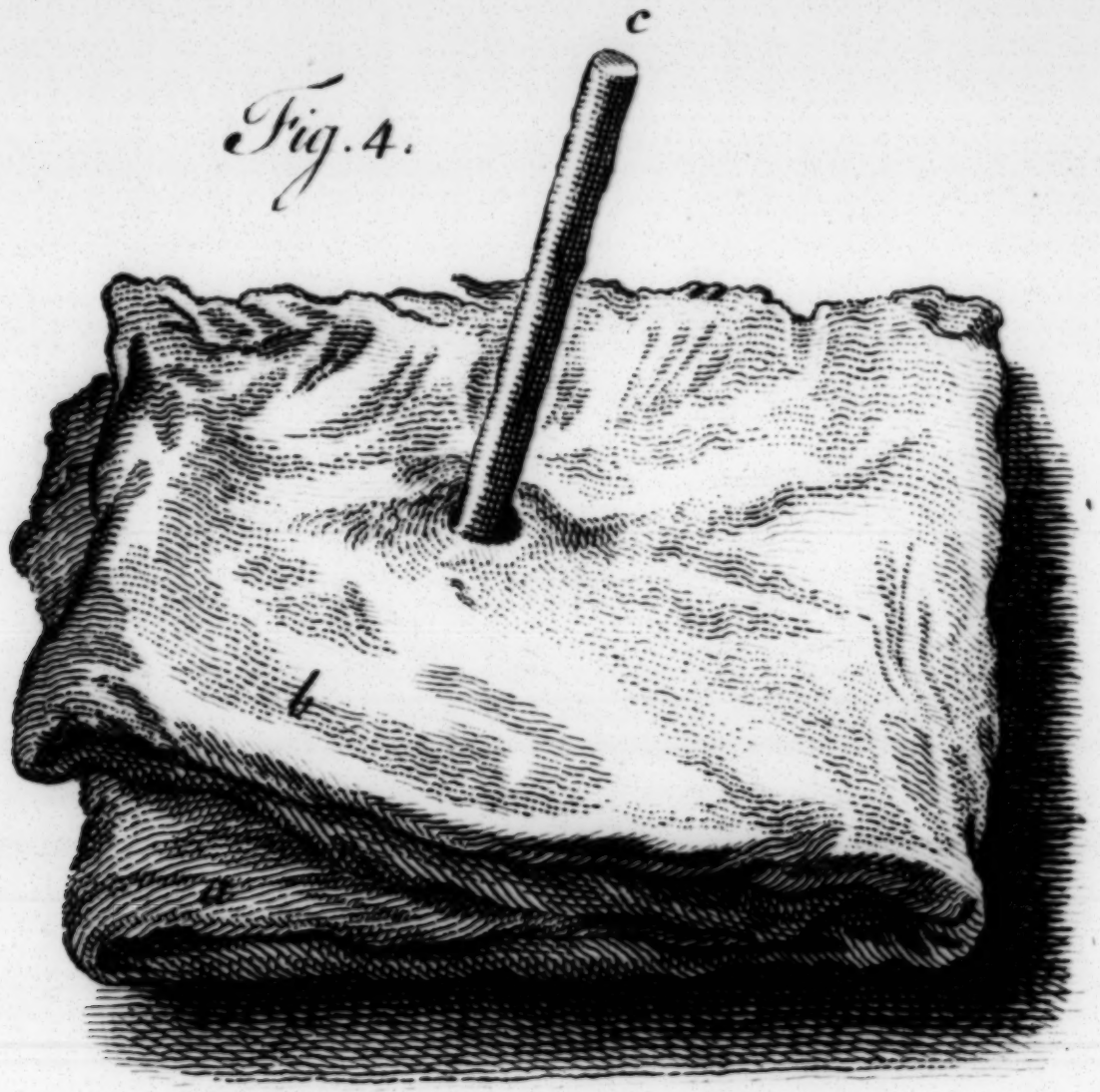
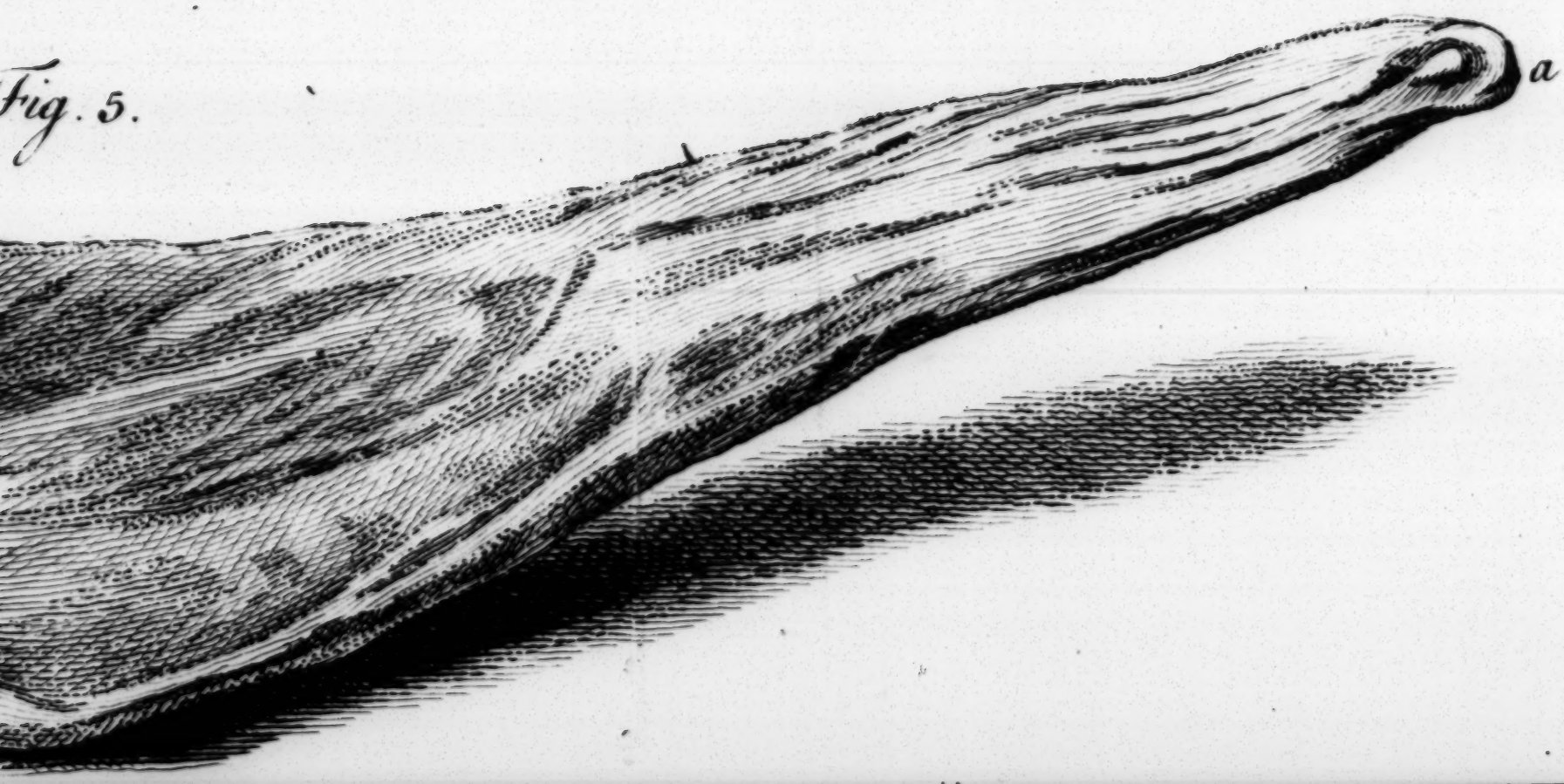


Fig. 5.



Basire sc.

EXPLANATION of the Figures; TAB. I.

Fig. 1. Short ſtaple iſinglaſs.
a The wooden peg, which faſtens the two ends of the iſinglaſs.
b The extremities folded inwards.

Fig. 2. The ſaw-knife.

Fig. 3. Long ſtaple iſinglaſs.
a b c Wooden pegs or pins.
d e f Three round ſticks, or pieces of wood, faſtened in the angles of the iſinglaſs by the pins *a* and *c*.

Fig. 4. Book iſinglaſs.
a b The two ſides, which reſemble the cover of a book.
c The wooden pin run through the ſides, to faſten them together.

Fig. 5. The entire ſound, or *veſica aeria*, of the iſinglaſs fiſh dried in its natural ſtate.
a The hole made for the packthread, wherewith it is hung up to dry.
b The orifice of part of the pneumatic veſſels left with the ſound.

II. *A Leter to* Charles Morton, *M. D. Sec. R. S. from Mr.* Adam Walker; *containing an Account of the Cavern of* Dunmore Park, *near* Kilkenny, *in* Ireland.

Dublin, April 26, 1771.

SIR,

Read Nov. 19, 1772.

AS I do not find in your Tranſactions any account of the cave of Dunmore Park, about three miles Weſt of Kilkenny, I beg leave to lay before your learned Society an account of this ſingular cavern, as near as an eye-ſurvey, and a few experiments on its ſtones and petrefactions, will admit. It is ſituated in a fine plain, riſing indeed here and there into ſmall hills. The country all round abounds with limeſtone, and quaries of beautiful black marble, variegated with white ſhells. Different from thoſe of Derbyſhire and Mendip, this cave deſcends perpendicularly 30 yards, from the top of a ſmall hill, through an opening 40 yards in diameter. The ſides of this pit are limeſtone-rock, whoſe chinks nouriſh variety of ſhrubs and trees, down which the inſpector muſt deſcend with great caution. In this deſcent, he is amuſed

amuſed with flights of wild pigeons, and jackdaws from the cave below. When he reaches the bottom, he ſees one ſide of this pit ſupported by a natural arch of rock, above 25 yards wide, under which he goes horizontally, and ſees two ſubterraneous openings to the right and left. If he turns to the right, he makes his way over rocks and ſtones, coated with ſpar in the moſt whimſical ſhapes, and formed from the dropping roof, juſt as the dripping of a candle would cover a pebble. Theſe knobs take a fine poliſh, are tranſparent, and variegated with the wildeſt aſſemblage of colouring. The Earl of Wandesford had one of them ſawn into a ſlab, and it is as beautiful as a Moco. When I tried theſe petrefactions with an acid, the efferveſcence was exceſſive ſtrong; and, as the earth all round is calcareous, and the ſtones limeſtone, I humbly apprehend the icicle figures impending from the roof, and theſe knobs, are thus formed. The rains, that fall on the hill over this cavern, oozing through an okery calcareous earth, and the limeſtone roof, imbibe or diſſolve their fine particles in their deſcent; and, as this mixture can only filter through the rock exceedingly ſlowly, the water hanging on the roof is ſoon diſſolved by the air, and the ſtony particles are left behind. Hence are formed the icicle-ſhaped cones that hang from the roof; theſe growing perpetually longer, have, in many parts of the cave, met the knobs from the bottom, and formed a number of fantaſtic appearances, like the pillars of a Gothic cathedral, organs, croſſes, &c. When the rain filters pretty faſt through the roof, it falls on the rocks below, and grows there into knobs and cones,

whoſe vertex points to thoſe that impend from the roof.

A ſpectator, viewing theſe, cannot but conceive himſelf in the mouth of a huge wild beaſt, with ten thouſand teeth above his head, and as many under his feet. The ſcene is indeed both pleaſing and awe-ful; the candles burning dim, from the moiſture in the air, juſt ſerved to ſhew a ſpangled roof perpetually varniſhed with water, in ſome places upwards of 20 yards high; in other places we crawled on all-four, through cells that will but admit one at a time. After having ſcrambled about 500 yards into this (which I will beg leave to call the) right-hand part of the cave, we returned to day-light, and then proceeded to view the left-hand part. Here, as our guides informed us there were many different branches of the cavern, we tied one ball of pack-thread to another, as we went forward, that we might more eaſily find our way back. This branch is not ſo horizontal as the other; it inclines downwards, and the openings in it are vaſtly wider, ſome being at leaſt 100 yards wide, and above 50 high. A ſmall rill accompanied us, which, by its different falls, formed a ſort of rude harmony, well ſuited to the place. In a ſtanding part of this brook, and near a quarter of a mile from the entrance, we found the bones of a hundred at leaſt of the human race; ſome were very large, but when taken out of the water, they crumbled away. As we could find nothing like an inſcription, or earth for a burying-place, we conjectured that ſome of the civil wars, perhaps that of 1641, might have driven the owners of theſe bones into this place. The tradition of the neighbourhood threw no light upon it.

Many

Many of the rocks on the roof and ſides of this cavern are black marble, full of white ſpots of a ſhell-like figure; and the whole neighbourhood is full of quarries of this beautiful ſtone, which takes a fine poliſh, and is uſed through the three kingdoms for ſlabs, chimney-pieces, &c. I obſerved, in ſome deep and wet parts of theſe quarries, this elegant foſſil in the firſt ſtages of its formation; the ſhells are real, but ſo ſoftened by time and their moiſt ſituation, as to be ſuſceptible of receiving the ſtony particles into their pores, by whoſe coheſive quality, they in time become thoſe hard white curls that give value to the marble: and it is very remarkable, and a proof that theſe white ſpots have been real ſhells, and thus formed, that the longer a chimney-piece or ſlab is uſed, the more of thoſe ſpots ripen into view.

I have taken many more notes of the natural curioſities in this kingdom, which I ſhall be happy to communicate to your reſpectable Society, if you think the ſubjects of ſufficient importance; and am,

With great reſpect,

SIR,

Your moſt obedient,

humble ſervant,

Adam Walker.

III. *A ſhort Account of ſome Specimens of native Lead found in a Mine of* Monmouthſhire: *In a Letter from* Michael Morris, *M. D. F. R. S. to* M. Maty, *M. D. Sec. R. S.*

Fludyer's-Street, Weſtminſter, Nov. 5, 1772.

SIR,

Read Nov. 19, 1772. ABOUT the middle of laſt July, I received three ſpecimens of lead-ore from Valentine Morris, Eſq; of Piercefield, in Monmouthſhire. They were dug up in one of his fields, on making ſome drains, at no conſiderable depth; they were marked N° 1, 2, 3. On reducing to powder an ounce and a half of the ore, marked N° 3, in order to aſſay it, I perceived that ſeveral ſmall bits were flatted by the peſtle, which, on a farther examination, proved to be native lead. Though the bits of lead are inconſiderable, yet, as they are the firſt that have been publickly ſeen in England, or, that I know of, in Europe, ſome of the beſt and lateſt writers on mineralogy declaring that they have not met with any, I thought it my duty to acquaint the Royal Society with the fact, that the firſt

account

account of native lead may appear in the Philoſophical Tranſactions, as well as the firſt account of native tin.

N. B. In more than 300 aſſays of lead ore, I have met with nothing of the kind before.

I am,

Yours. &c.

M. Morris.

IV. *Farther Remarks upon a Denarius of the* Veturian *Family, with an* Etruſcan *Inſcription on the Reverſe, formerly conſidered. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Reverend* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Received June 20, 1772.

SIR,

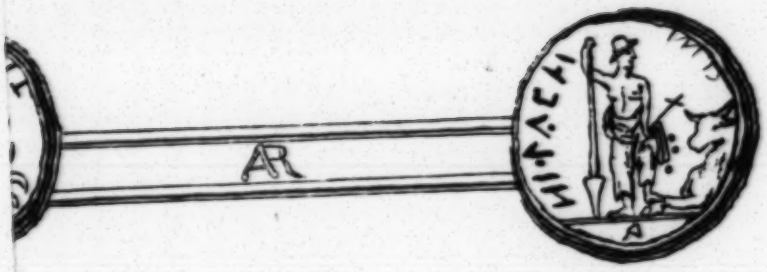
I.

Read Nov. 19, 1772. SOME years ſince (1), I offered my thoughts upon an inedited Samnite denarius, in my ſmall collection, with [See TAB. II. n. 1.] the Samnite-Etruſcan letters 𐌅𐌕𐌅𐌉·𐌉𐌇, as I then apprehended, upon the reverſe. But as the two laſt letters were ill preſerved, or rather in part defaced, I was not intirely ſatisfied with my lection of the

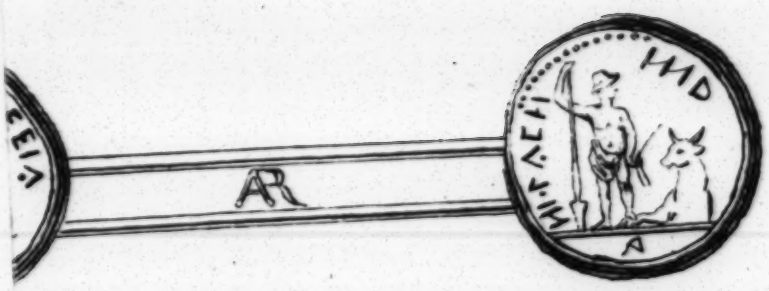
(1) *Philoſoph. Tranſact.* Vol. LVIII. p. 253—261.

inſcription

Tab. II. n. 1.

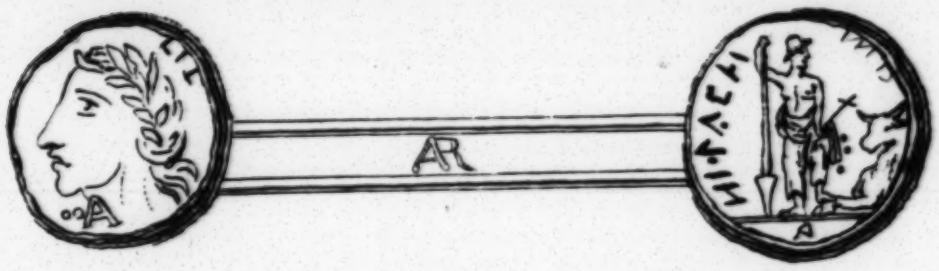


Tab. II. n. 2.

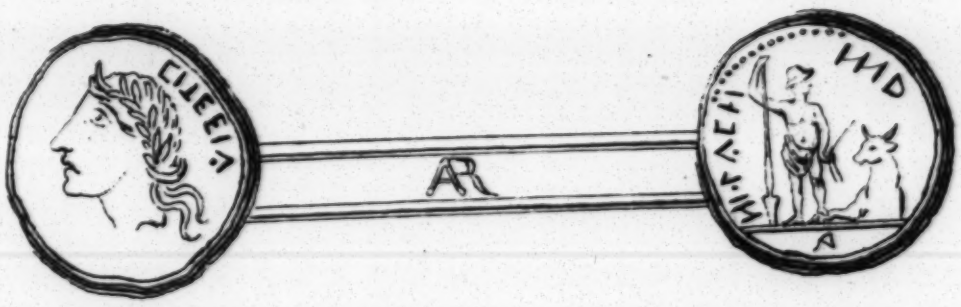


Basire sc.

Tab. II. n. 1.



Tab. II. n. 2.



Basire sc

2

inſcription to which they appertained. I have, however, ſince met with the [See TAB. II. n. 2.] ſame coin, finely preſerved, in the very valuable cabinet of the Reverend and learned Dr. Milles (*), Dean of Exeter, Preſident of the Society of Antiquaries, and Fellow of the Royal Society, with three letters, in the room of the two ſuppoſititious ones, upon it, perfectly formed; by the aſſiſtance of which, I have been enabled to give the true reading of the inſcription, and to arrive, I would flatter myſelf, at a full and complete interpretation of it. In order, therefore, to rectify all former miſtakes, ariſing from the injury received by the three laſt elements from time, I ſhall now beg leave to tranſmit this ſhort paper to you, to be laid before the Royal Society; not doubting but it will meet with the ſame candid acceptance, the ſame favourable reception, with which my other papers have been honoured, through the whole courſe of my correſpondence with you and two of your predeceſſors, from that very learned and moſt illuſtrious body.

(*) Towards the cloſe of May, 1771, the Dean ſent me a draught of the Samnite coin conſidered in this paper, very accurately taken; ſoon after which, the original inſcription on the reverſe of the medal, by an unlucky accident, was totally defaced. However, by the aſſiſtance of the draught ſent me, and my ſimilar coin, on which the remains of the three injured letters were rendered more viſible, by the help of a pretty good glaſs, and an attentive compariſon of them with the correſpondent elements on the draught the Dean communicated to me, the inſcription exhibited by my medal, as well as the other given here, approaches the truth as near as poſſible.

II.

I am then fully convinced from the Samnite, or Samnite-Etruſcan, inſcription formerly viſible on the reverſe of the Dean's very valuable denarius, which I ſhall beg leave to conſider here, that the true legend exhibited by my coin is 𐌃𐌌𐌅𐌉𐌓𐌅𐌋·𐌉𐌍, NI. LVFII, or LVVII, MER, equivalent to NI. LVFIVS, or LVVIVS, MERRISS, MERRIX, or MEDDIX. I ſay equivalent to LVFIVS, or LVVIVS, becauſe the Etruſcan, or Samnite-Etruſcan, letter 𐌅 is endued with the power of V (2), as well as that of F, even (3) on this ſpecies of Samnite coins; and the Samnite termination II ſeems ſometimes at leaſt to have anſwered to the Roman, or Latin, IVS, as has been formerly obſerved. Theſe points are clearly evinced by the authors here referred to, as well as others, that might, with equal facility, be produced. As for the elements 𐌃𐌌, they very probably repreſented the ſyllable MER; nothing being more common than the ſuppreſſion of a vowel between two conſonants, in antient Etruſcan words, as I have elſewhere demonſtratively proved (4). To the inſtances there mentioned I might add many

(2) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 414, 415. *Philoſoph. Tranſact.* Vol. LII. Par. I. p. 28—39.

(3) *Philoſoph. Tranſact.* ubi ſup. & Vol. LI. Par. II. p. 853—865. Anton. Franciſc. Gor. ubi ſup. & alib. Sig. Olivier. in *Sag. di Diſſertaz. di Corton.* Tom. II. p. 49—73. & Tom. IV. p. 133—149. *Philoſoph. Tranſact.* Vol. LII. Par. I. p. 34, 35. Lond. 1762.

(4) *Philoſoph. Tranſact.* Vol. LXI. p. 85, 86, 87.

others,

others, were it in any manner neceſſary. But as this is not the caſe, it will be ſufficient to refer to the paper (5) wherein the former inſtances are contained.

III.

That the Samnite letters 𐌌𐌓 ſtood (*) for the word MERRISS, or MERRIX, uſed at Herculaneum, and the neighbouring part of Campania, for MEDDIX, denoting the chief magiſtrate of the Oſcans and Samnites, as we learn from Ennius (6) and Feſtus (7), will not be denied, as I apprehend, by

(5) *Philoſoph. Tranſact.* ubi ſup.

(*) The Samnites and Etruſcans by a ſingle ſyllable nŏt ſeldom expreſſed a whole word on their coins. Thus for 𐌀𐌍𐌅𐌋, LVNA, we find 𐌍𐌅𐌋, LVN, or 𐌅𐌋, LV, on the pieces ſtruck in the city of LVNA; 𐌅𐌕, TV, for TVTERE, on thoſe of TVDER, or TODI; 𐌀𐌋𐌕, TLA, for TELAMON, on the medals attributed to the city going under that name, at preſent called TELAMONE; 𐌉𐌀𐌆, FAI, on thoſe of FÆSVLÆ, now denominated FIESOLE, to omit many other ſimilar inſtances that occur. And as the Romans on their conſular coins exhibited COS for CONSVL, DICT for DICTATOR, &c. the Samnites and Oſcans on their medals probably uſed 𐌌𐌓, MR, or MER, for MERRISS, or MEDDIX, the ſupreme magiſtrate, or rather one of the two ſupreme magiſtrates, of the city, or country, where the pieces I am conſidering were ſtruck. Hence it ſeems to appear, that the Romans borrowed the ſhort manner of writing here mentioned either of the Samnites, the Oſcans, or the proper Etruſcans. *Sag. Diſſertaz. di Corton.* Tom. II. p. 53. 41. *Philoſ. Tranſact.* Vol. LIV. p. 100. 103. *Una Lettera Al Sig. Abat. Barthelemy di* Annibale degli Abati Oliveri, &c. p. 43. In Peſaro, 1757.

(6) Ennii fragm. in *Annal.* viii. Vid. etiam not. Franciſc. Heſſelii in loc. p. 147, 148. Amſtelodami, 1707.

(7) Feſt. in voc. *Meddix.*

any one who has read what has been advanced on this head by Sig. Avvocato (8) Paſſeri. This magiſtrate is called (*) Meddixtuticus by (9) Livy. NI. LVFIUS, or LVVIVS, therefore, ſeems not to have been one of the Italian generals in the Social War, as (10) I formerly ſuppoſed, but one of the chief magiſtrates either of the Oſcans or the Samnites, coeval with that war; there having (11) been two ſuch magiſtrates, anſwering to the two Roman Conſuls, and the two Carthaginian Suffetes, in both thoſe nations. That D inverted, ɑ, amongſt the antient Etruſcans, and the Samnites, had the power of R, whence it came to paſs that theſe two elements were looked upon as nearly related to each other, and R not infrequently uſed for D, we learn from the (12) famous Matthæus Ægyptius and Father (13) Gori. We are not therefore to be

(8) Joan. Baptiſt. Paſſer. Piſaurenſ. *Junonal. Sacr. Menſ. Herculanenſ.* in *Symbol. Litterar.* &c. Vol. I. p. 209, 210. Florentiæ, 1748.

(*) The Meddixtuticus, or Mediaſtuticus, of Livy ſeems to be formed of the two Samnite-Etruſcan words ƧƎIDDEM, or ƧKIHTƆVT, ƧƎIDDIKƧ, MERRISS, MERRICKS, or MEDDIX, TVFTICKS, or TVTICKS, not TVCTICKS, as Sig. Paſſeri has written and pronounced it; both which words ſeem to denote the ſupreme magiſtrate, or rather one of the two ſupreme magiſtrates, amongſt the Oſcans and the Samnites. Paſſer. ubi ſup. p. 210—213.

(9) Liv. Lib. xxvi. c. 6.

(10) *Philoſoph. Tranſact.* Vol. LVIII. p. 258.

(11) Jo. Bapt. Paſſer. Piſaurenſ. ubi ſup.

(12) Matthæus Ægypt. in *Explicat. S. C. Romanor. de Bacchanal.* p. 148, 149. Neapoli, 1729.

(13) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 412, 413. Florentiæ, 1737.

ſurprized

ſurprized that MERRISS, MERRIX, and MEDDIX, ſhould be conſidered as the ſame word, or at leaſt as words of the ſame ſignification, by the celebrated Sig. (14) Giovanni Battiſta Paſſeri; who has ſet the point I am now upon, as well as every thing he has advanced relative to it, in the ſtrongeſt and cleareſt light.

IV.

That the Luvian, or Luſian, family was ſettled in that part of Campania poſſeſſed by the Oſcans and Samnites, or at leaſt almoſt contiguous to it, may be deemed highly probable, from the following inſcription (15), found near Venafro, the antient Venatrum, at no very great diſtance from Herculaneum, where the word MERRISS, or MERRIX, for MEDDIX, was antiently uſed, and now to be ſeen near that city (16).

LVVIA M. F. POSTVMA
SIGNVM. ET BASIM. D.
IVNONI. REG. SACRVM.

Whence, in conjunction with what has been already obſerved, we may conclude, that the coins I have been offering my thoughts upon may be preſumed to have made their firſt appearance amongſt either the Samnites or the Oſcans, probably the former, ſettled in a part of Campania, at no very great diſtance from Herculaneum; and that Ni. Lu-

(14) J. Bapt. Paſſer. Piſaurenſ. ubi ſup.
(15) Ludovic. Anton. Murator. *Theſaur. veter. inſcript.* &c. Tom. I. P. xvi. n. 4. Mediolani, 1739.
(16) Idem ibid.

vius, or Lufius, was the Merriſs, Merrix, or Meddix, or at leaſt one of the two magiſtrates going under that denomination, of the city, if not the whole country, where it was ſtruck.

V.

With regard to the year, as well as the place, wherein the medals in view firſt appeared, neither the one nor the other can, with preciſion, be aſcertained; though the Latin A on one ſide of my denarius and the Etruſcan 𐌀 on the reverſe, in the exergue, ſeem to point at the initial letter of the name of the place where they were ſtruck. And that this operation happened about the time of the Social War, we may pronounce highly probable, from the agreement of theſe denarii in moſt particulars with other Samnite coins, undoubtedly ſtruck not long after (*) the commencement of that war.

VI.

It would be wholly unneceſſary, and altogether immaterial, to mention the different explications of the ſymbol on the reverſes of theſe medals, offered by ſeveral learned men, here; as ſcarce any of them ſeems to be perfectly agreeable to truth, or to be ſuch as may be abſolutely depended upon. I ſhall therefore content myſelf with obſerving, that ſome light is thrown upon this intricate ſubject

(*) This ſeems likewiſe, from what has been already obſerved in a former paper, pretty clearly to appear. *Philoſoph. Tranſact.* Vol. LVIII. p. 256, 257.

by

by a curious paſſage in (17) Strabo; that the two writers who have handled this matter in the moſt copious, and learned, manner are Sig. (18) Bianconi and (19) M. Pellerin; that the medals I have been conſidering have notified to us the civil office, or dignity, of Ni. Luvius, or Lufius, as ſome other ſimilar Samnite denarii have done the military poſt of (20) Papius Mutilus; and that the name of the Lufian family has never yet appeared, as I apprehend, on any other ancient coins. I ſhall only beg leave to add, that I remain, with all poſſible conſideration and eſteem,

SIR,

Your moſt obliged,

and moſt obedient,

humble ſervant,

Chriſt-Church, Oxon.
June 18, 1772.

John Swinton.

(17) Strab. *Geogr.* Lib. v. p. 250. Lutetiæ Pariſiorum, 1620.
(18) Joan. Bapt. Biancon. *de Priſc. Hebræor. et Græcor. Lit. Libel.* p. 73. Bononiæ, 1748.
(19) *Supplem. aux Recueils des Medaill.* p. 11, 12. A Paris, 1765.
(20) *Philoſoph. Tranſact.* Vol. LI. Par. II. p. 857. Vol. LII. Par. I. p. 28—39. & Vol. LIX. p. 432—444. Lond. 1769.

V. *A Catalogue of the Fifty Plants, from* Chelſea *Garden, preſented to the* Royal Society, *by the Worſhipful Company of Apothecaries for the Year* 1771, *purſuant to the Direction of the late Sir* Hans Sloane, *Bart. M. D. Soc. Reg. nuper Præſes: By* Staneſby Alchorne, *Member of the ſaid Society of Apothecaries in* London.

Read Nov. 26, 1772.

2451 ACER, *Pſeudo-Platanus*, foliis quinque lobis inæqualiter ſerratis, floribus racemoſis. Lin. Spec. plant. 1495. 2.
Acer montanum candidum. Bauh. pin. 430.

2452 Æſculus, *Hippo-Caſtanum*, floribus heptandris. Lin. Spec. plant. 488. 1.
Caſtanea folio multifido. Bauh. pin. 419.

2453 Andromeda, *Polifolia*, pedunculis aggregatis, corollis ovatis, foliis alternis lanceolatis revolutis. Lin. Spec. plant. 564. 5.
Erica humilis roſmarini foliis, unedonis flore, capſula ciſtoide. Pluck. alm. tab. 175. fig. 3.

2454 Aquilegia, *vulgaris*, nectariis incurvis. Lin. Spec. plant. 752. 1. α.

Aquilegia

Aquilegia ſylveſtris. Bauh. pin. 144.

2455 Arbutus, *Andrachne*, caule arboreo, foliis glabris integerrimis, baccis polyſpermis. Lin. Spec. plant. 566. 2.

Arbutus folio non ſerrato. Bauh. pin. 460.

2456 Artemiſia, *Dracunculus*, foliis lanceolatis glabris integerrimis. Lin. Sp. plant. 1189. 19.

Dracunculus hortenſis. Bauh. pin. 98.

2457 Artemeſia, *vulgaris*, foliis pinnatifidis planis inciſis; ſubtus tomentoſis; racemis ſimplicibus, floribus ovatis, radio quinquefloro. Lin. Spec. plant. 1189. 16.

Artemiſia vulgaris major. Bauh. pin. 137.

2458 Betula, *nana*, foliis orbiculatis crenatis. Lin. Spec. plant. 1394. 4. Flor. Lap. tab. 6. fig. 4.

Betula pumila. Laef. Pruſſ. 10.

2459 Caltha, *paluſtris*. Lin. Spec. plant. 784. 1. α.

Caltha paluſtris flore ſimplici. Bauh. pin. 276.

2460 Capſicum, *annuum*, caule herbaceo, pedunculis ſolitariis. Lin. Spec. plant. 270. 1.

Piper Indicum vulgatiſſimum. Bauh. pin. 102.

2461 Capſicum, *fruteſcens*, caule fruticoſo, pedunculis geminis. Lin. Spec. plant. 271. 2. β.

Capſicum minus, fructu parvo pyramidali erecto. Sloan. Hiſt. I. 240. tab. 146. fig. 2.

2462 Centaurea, *benedicta*, calycibus duplicato-ſpinoſis lanatis involucratis, foliis ſemidecurrentibus

rentibus denticulato-ſpinoſis. Lin. Spec. plant. 1296. 42.

Carduus ſylveſtris hirſutus, ſ. Carduus benedictus. Bauh. pin. 378.

2463 Cupreſſus, *ſempervirens*, foliis imbricatis, frondibus quadrangulis. Lin. Spec. plant. 1422. 1. α.

Cupreſſus. Bauh. pin. 488.

2464 Daphne, *Laureola*, racemis axillaribus, foliis lanceolatis glabris. Lin. Spec. plant. 510. 6.

Laureola ſempervirens flore viridi, quibuſdam Laureola mas. Bauh. pin. 662.

2465 Daphne, *Mezereum*, floribus ſeſſilibus ternis caulinis, foliis lanceolatis deciduis. Lin. Spec. plant. 509. 1.

Laureola folio deciduo, flore purpureo, officinis Laureola fœmina. Bauh. pin. 462.

2466 Dirca, *paluſtris*. Lin. Spec. plant. 512. 1.

Thymelæa floribus albis primo vere erumpentibus, foliis oblongis acuminatis, viminibus & cortice valde tenacibus. Geor. Virg. 155.

2467 Erica, *daboecii*, racemo terminali, foliis alternis ſubtus tomentoſis. Lin. Spec. plant. 509. 38.

Erica Hibernica, foliis myrti piloſis ſubtus incanis. Pet. gaz. 42. tab. 27. fig. 4.

2468 Fragaria, *ſterilis*, caule decumbente repente. Lin. Spec. plant. 709. 3.

Fragaria ſterilis. Bauh. pin. 327.

2469 Gardenia, *Florida*. Lin. Spec. plant. 305. 1.

Jaſminum, foliis lanceolatis oppoſitis integerrimis, calycibus acutioribus. Miller's Icons, tab. 180.

2470 Geniſta, *Anglica*, ſpinis ſimplicibus, ramis floriferis inermibus, foliis lanceolatis. Lin. Spec. plant. 999. 11.
Geniſta minor aſpaltoides. Bauh. pin. 395.

2471 Heuchera, *Americana*. Lin. Spec. plant. 328. 1.
Sanicula, ſ. Cortuſa Americana ſpicata, floribus ſqualide purpureis. Pluk. alm. tab. 58. fig. 3.

2472 Hyſſopus, *officinalis*, ſpicis fœcundis. Lin. Spec. plant. 796. 1. α.
Hyſſopus officinarum cærulea, ſ. ſpicata. Bauh. pin. 217.

2473 Jaſminum, *officinale*, foliis oppoſitis pinnatis. Lin. Spec. plant. 9. 1.
Jaſminum vulgatius flore albo. Bauh. pin. 397.

2474 Knautia, *orientalis*. Lin. Spec. plant. 146.
Scabioſa orientalis, caryophylli flore. Vaill. act. 1772. p. 241.

2475 Liguſticum, *Leviſticum*, foliis multiplicibus, foliolis ſuperne inciſis. Lin. Spec. plant. 359. 1.
Liguſticum vulgare. Bauh. pin. 157.

2476 Medicago, *lupulina*, ſpicis ovalibus, leguminibus reniformibus monoſpermis, caule procumbente. Lin. Spec. plant. 1097. 7.
Triſolium pratenſe luteum capitulo breviore. Bauh. pin. 228.

2477 Mercurialis, *annua*, caule brachiato, foliis glabris, floribus ſpicatis. Lin. Spec. plant. 1465. 3.
Mercurialis teſticulata, ſ. mas. & Mercurialis ſpicata, ſ. fœmina. } Bauh. pin. 121.

2478 Mercurialis, *perennis*, caule ſimpliciſſimo foliis ſcabris. Lin. Spec. plant. 1465. 1.
Mercurialis montana teſticulata, & Mercurialis montana ſpicata. } Bauh. pin. 122.

2479 Petiveria, *alliacea*, floribus hexandris. Lin. Spec. plant. 486. 1.
Verbenæ aut Scorodoniæ affinis anomala, flore albido, calyce aſpero, allii odore. Sloan. Hiſt. I. 172.

2480 Pæonia, *fœmina*, foliolis oblongis. Lin. Sp. plant. 747. 1. α.
Pæonia communis, ſ. fœmina. Bauh. pin. 323.

2481 Pteris, *aquilina*, frondibus ſupradecompoſitis, foliolis pinnatis: pinnis lanceolatis: infimis pinnatifidis, ſuperioribus minoribus. Lin. Spec. plant. 1533.
Filix ramoſa major, pinnulis obtuſis non dentata. Bauh. pin. 357.

2482 Quercus, *Ilex*, foliis ovato-oblongis indiviſis ſerratiſque, petiolatis, ſubtus incanis, cortice integro. Lin. Spec. plant. 1412. 3. β.
Ilex folio anguſto non ſerrato. Bauh. pin. 424.

2483 Quercus, *Suber*, foliis ovato-oblongis indiviſis ſerratis ſubtus tomentoſis, cortice ramoſo fungoſo. Lin. Spec. plant. 1413. 5.
Suber latifolium ſempervirens. Bauh. pin. 424.

2484 Ra-

2484 Ranunculus, *Auricomus*, foliis radicalibus reniformibus crenatis incisis, caulinis digitatis linearibus, caule multifloro. Lin. Spec. plant. 775. 13.
Ranunculus nemorosus, s. sylvaticus, folio subrotundo. Bauh. pin. 178.

2485 Ranunculus, *Flammula*, foliis ovato-lanceolatis petiolatis, caule declinato. Lin. Spec. plant. 773. 1. α.
Ranunculus longifolius palustris minor. Bauh. pin. 180.

2486 Rheum, *palmatum*, foliis palmatis acuminatis. Lin. Spec. plant. 531. 3.
Rhabarbar. Brun. orient. 192. tab. 73.

2487 Rubus, *arcticus*, foliis ternatis, cane inermi unifloro. Lin. Spec. plant. 708. 11.
Rubus caule unifloro, foliis ternatis. Lin. Fl. Lap. 207. tab. 5. fig. 2.

2488 Ruscus, *Androgynus*, foliis margine floriferis. Lin. Spec. plant. 1474.
Ruscus latifolius è foliorum sinu florifer & baccifer. Dil. Hort. Elth. 330. tab. 250. fig. 332.

2489 Salvia, *nemorosa*, foliis cordato-lanceolatis serratis planis, bracteis coloratis, corollæ labio infimo reflexo. Lin. Spec. plant. 35. 10.
Horminum sylvestre, salvifolium minus. Bauh. pin. 239.

2490 Scutellaria, *albida*, foliis subcordatis serratis rugosis opacis, spicis secundis bracteis ovatis. Lin. Mantissa. 248.
Scutellaria Teucrii facie flore albo. Bauh. hist. III. 291.

2491 Serratula, *ſpicata*, foliis linearibus baſi ciliatis, floribus ſpicatis ſeſſilibus lateralibus, caule ſimplici. Lin. Spec. plant. 1147. 11.
Jacca anguſtifolia, tuberoſa radice Virginiana. Pluk. alm. tab. 424. fig. 6.

2492 Sibthorpia, *peregrina*, foliis reniformibus crenatis, pedunculis geminis. Lin. Spec. plant. 880. 3.
Planta anonyma. Pluk phyt. tab. 257. fig. 5.

2493 Staphylea, *pinnata*, foliis pinnatis. Lin. Spec. plant. 386. 1.
Piſtacia ſylveſtris. Bauh. pin. 401.

2494 Taxus, *baccata*, foliis approximatis. Lin. Spec. plant. 1472. 1.
Taxus. Bauh. pin. 505.

2495 Teucrium, *hircanicum*, foliis cordato-oblongis obtuſis, caule brachiato dichotomo, ſpicis longiſſimis terminalibus ſeſſilibus ſpiralibus. Lin. Spec. plant. 789. 16.
Teucrium foliis cordatis crenatis petiolatis, ſpicis oblongis denſiſſimis. Arduin. Spec. 13. tab. 4.

2496 Teucrium, *Scordium*, foliis oblongis ſeſſilibus dentato-ſerratis, floribus geminis lateralibus pedunculatis, caule diffuſo. Lin. Spec. plant. 790. 20.
Scordium. Bauh. pin. 247.

2497 Thymus, *Acinos*, verticillis ſexfloris, caulibus erectis ſubramoſis, foliis acutis ſerratis. Lin. Spec. plant. 826. 4.
Clinopodium arvenſe, ocymi facie. Bauh. pin. 225.

2498 Thy-

2498 Thymus, *Serpyllum*, floribus capitatis, caulibus decumbentibus, foliis planis obtuſis, baſi ciliatis. Lin. Spec. plant. 825. 1. ε.
Serpyllum foliis citri odore. Bauh. pin. 220.

2499 Thymus, *vulgaris*, erectus, foliis revolutis ovatis, floribus verticillato-ſpicatis. Lin. Spec. plant. 825. 2. α.
Thymus vulgaris folio tenuiore. Bauh. pin. 219.

2500 Viola, *odorata*, acaulis, foliis cordatis, ſtolonibus reptantibus. Lin. Spec. pl. 1324. 8.
Viola martia purpurea, flore ſimplici odore. Bauh. pin. 199.

VI. *Extract of a Letter from Mr.* Ebenezer Kinnerſley *to* Benjamin Franklin, *L L. D. F. R. S. on ſome electrical Experiments made with Charcoal.*

Received Auguſt 16, 1772.

Philadelphia, October 13, 1770.

Read Dec. 10, 1772. THE conducting quality of ſome ſorts of charcoal is indeed very remarkable. I have found oak, beech, and maple, to conduct very well; but tried ſeveral pieces of pine coal, without finding one that would conduct at all; perhaps they were made in a fire not hot enough, or not continued in it long enough. A ſtrong line drawn on paper with a black lead pencil, will conduct an electrical ſhock pretty readily; but this, perhaps, may not be new to you.

On the 12th of laſt July, three houſes in this city, and a ſloop at one of the wharfs, were, in leſs than an hour's time, all ſtruck with lightning. The ſloop, with two of the houſes, were conſiderably damaged; the other was the dwelling-houſe of Mr. Joſeph Moulde, in Lombard-ſtreet, which was provided with

with a round iron conductor, half an inch thick, its ſeveral lengths ſcrewed together, ſo as to make very good joints, and the lower end five or ſix feet under ground; the lightning, leaving every thing elſe, purſued its way through that, melted off ſix inches and a half of the ſlendereſt part of a braſs wire fixed on the top, and did no further damage within doors, or without. Captain Falconer, who brings you this, was in the houſe at the time of the ſtroke, and ſays it was an aſtoniſhing loud one.

VII. *Account*

VII. *Account of an Experiment made with a Thermometer, whoſe Bulb was painted black, and expoſed to the direct Rays of the Sun: In a Letter from* Richard Watſon, *D. D. Regius Profeſſor of Divinity at* Cambridge, *and F. R. S. to* Mathew Maty, *M. D. Sec. R. S.*

Heverſham, September 18, 1772.

DEAR SIR,

Read Dec. 1, 1772. DURING the hot weather, which we had in the latter end of June and the beginning of July laſt, I made an experiment at Cambridge, which I then thought no more of, but which an accident hath brought to my mind again; and I now venture to ſend you an account of it, in hopes that ſome of your philoſophical friends will take the trouble of proſecuting it. I expoſed the bulb of an excellent thermometer to the direct rays of the Sun, when the ſky was perfectly free from clouds: the mercury roſe to 108° of Fahrenheit's ſcale, and continued ſtationary. A fancy ſtruck me, to give the bulb a black covering; this was eaſily effected by a camel's hair pencil and Indian ink; the mercury

mercury ſunk a few degrees during the application of the coating, and the evaporation of the water; but preſently after roſe to 118°, or 10° in conſequence of the black coat with which I had covered that part of the bulb which was expoſed to the Sun. If the bulbs of ſeveral correſponding thermometers were painted of different colours, and expoſed at the ſame time to the Sun, for a given period, ſome conjectures, reſpecting the diſpoſition of the ſeveral primary colours for receiving and retaining heat, might be formed, which could not fail of being, in ſome degree, intereſting.

I am,

DEAR SIR,

Your moſt obedient ſervant,

Richard Watſon.

VIII. *A Report of the Committee appointed by the Royal Society, to conſider of a Method for ſecuring the Powder Magazines at* Purfleet.

To the PRESIDENT and MEMBERS of the ROYAL SOCIETY.

GENTLEMEN,

THE Society being conſulted by the Board of Ordnance, on the propriety of fixing conductors for ſecuring the powder magazines at Purfleet from lightning, and having thereupon done us the honour of appointing us a committee, to conſider the ſame, and report our opinion; we have accordingly viſited thoſe buildings, and examined, with care and attention, their ſituation, conſtruction, and circumſtances, which we find as follows.

They are five in number, each about 160 feet long, and about 52 feet wide, built of brick, arched under the roof, which in one of them is ſlated, with a coping of lead 22 inches wide on the ridge from end to end; and the others, as we were informed, are

are ſoon to be covered in the ſame manner. They ſtand parallel to each other at about 57 feet diſtance, and are founded on a chalk rock, about 100 feet from the river, which riſes in high tides within a few inches of the level of the ground, its brackiſh water alſo ſoaking through to the wells that are dug near to the buildings.

The barrels of powder, when the magazines are full, lie piled on each other up to the ſpring of the arches; and there are four copper hoops on each barrel, which, with a number of perpendicular iron bars, (that came down through the arches, to ſupport a long grooved piece of timber, wherein the crane was uſually moved and guided to any part where it was wanted) formed broken conductors within the building, the more dangerous from their being incompleat, as the exploſion from hoop to hoop, in the paſſage of lightning drawn down through the bars among the barrels, might eaſily happen to fire the powder contained in them. But the workmen were removing all thoſe iron bars (by the advice of ſome members of this Society, who had been previouſly conſulted); a meaſure we very much approve of.

On an elevated ground, nearly equal in height with the tops of the magazines, and 150 yards from them, is the houſe where the Board uſually meet. It is a lofty building, with a pointed hip-roof, the copings of lead down to the gutters, from which leaden pipes deſcend at each end of the building into the water of wells of 40 feet deep, for the purpoſe of conveying water forced up by engines to a ciſtern

in the roof. There is alſo a proof-houſe, adjoining to the end of one of the magazines, and a clock-houſe, at the diſtance of feet from them, which has a weathercock on an iron ſpindle, and, probably, ſome incompleat conductors within, ſuch as the wire uſually extending up from a clock to its hammer, the clock, pendulum, rod, &c.

The blowing up of a magazine of gun-powder by lightning, within a few years paſt, at Breſcia in Italy, which demoliſhed a conſiderable part of the town, with the loſs of many lives, does, in our opinion, ſtrongly urge the propriety of guarding ſuch magazines from that kind of danger; and ſince it is now well known, from many obſervations, that metals have the property of conducting lightning, and a method has been diſcovered of uſing that property for the ſecurity of buildings, by ſo diſpoſing and fixing iron rods, as to receive, and convey away, ſuch lightning as might otherwiſe have damaged them; which method has been practiſed near twenty years in many places, and attended with ſucceſs, in all the inſtances that have come to our knowledge, we cannot, therefore, but think it adviſeable to provide conductors of that kind for the magazines in queſtion.

In common caſes, it has been judged ſufficient, if the lower part of the conductor were ſunk three or four feet into the ground, till it came to moiſt earth; but this being a caſe of the greateſt importance, we are of opinion that greater precaution ſhould be taken. Therefore, we would adviſe, that, at each end of each magazine, a well ſhould be dug

in

in or through the chalk, ſo deep as to have in it at leaſt four feet of ſtanding water. From the bottom of this water ſhould ariſe a piece of leaden pipe, to or near the ſurface of the ground, where it ſhould be ſtrongly joined to the end of an upright iron bar, an inch and half diameter, faſtened to the wall by leaden ſtraps, and extending ten feet above the ridge of the building, tapering from the ridge upwards to a ſharp point, the upper 12 inches of copper, the iron to be painted.

We mention lead for the under-ground part of the conductor, as leſs liable to ruſt in water and moiſt places; in the form of a pipe, as giving greater ſtiffneſs for the ſubſtance; and iron for the part above-ground, as ſtronger, and leſs likely to be cut away. The pieces, of which the bar may be compoſed, ſhould be ſcrewed ſtrongly into each other, by a cloſe joint, with a thin plate of lead between the ſhoulders, to make the joining or continuation of the metal more perfect. Each rod, in paſſing above the ridge, ſhould be ſtrongly and cloſely connected by iron or lead, or both, with the leaden coping of the roof, whereby a communication of metal will be made between the two bars of each building, for a more free and eaſy conducting of the lightning into the earth.

We alſo adviſe, in conſideration of the great length of the buildings, that two wells, of the ſame depth with the others, ſhould be dug within twelve feet of the doors of the two outſide magazines; that is to ſay, one of them on the north ſide of the north building, the other on the ſouth ſide of the ſouth build-

building; from the bottom of which wells, ſimilar conductors ſhould be carried up to the eaves, there joining well with a plate of lead, extending on the roof up to the leaden coping of the ridge, the ſaid plate of lead being of equal ſubſtance with that of the coping.

We are further of opinion, that it will be right to form a communication of lead from the top of the chimney of the proof-houſe to the lead on its ridge, and thence to the lead on the ridge of the corridor, and thence to the iron conductor of the adjacent end of the magazine; and alſo to fix a conductor from the bottom of the weather-cock ſpindle of the clock-houſe, down on the outſide of that building, into the moiſt earth.

As to the board-houſe, we think it already well furniſhed with conductors, by the ſeveral leaden communications above-mentioned, from the point of the roof down into the water, and that, by its height and proximity, it may be ſome ſecurity to the building below it; we therefore propoſe no other conductor for that building, and only adviſe erecting a pointed iron rod on the ſummit, ſimilar to thoſe before deſcribed, and communicating with thoſe conductors.

To theſe directions we would add a caution, that, in all future alterations or repairs of the buildings, ſpecial care be taken that the metalline communications be not cut off or removed.

It remains that we expreſs our acknowledgements to Sir Charles Frederick, Surveyor-general of the Ordnance, for the obliging attention with which he entertained

entertained and accommodated us on the day of our enquiry.

With very great reſpect, we are,

GENTLEMEN,

Your moſt obedient,

humble ſervants,

Auguſt 21, 1772.

H. Cavendiſh,
William Watſon,
B. Franklin,
J. Robertſon.

Mr.

Mr. WILSON's *Diſſent to Part of the preceding Report.*

I Diſſent from the Report above, in that part only which recommends that each conductor ſhould terminate in a *point*.

My reaſon for diſſenting is, that ſuch conductors are, in my opinion, leſs ſafe than thoſe which are not *pointed*.

Every *point*, as ſuch, I conſider as *ſollicit*ing the lightning, and, by that means, not only contributing to *increaſe* the quantity of every actual diſcharge, but alſo frequently occaſioning a diſcharge where it might not otherwiſe have happened.

If, therefore, we invite the lightning, while we are ignorant what the quantity, or the effects of it, may be, we may be *promoting* the very miſchief we mean to prevent.

Whereas if, inſtead of pointed, we make uſe of blunted conductors, thoſe will as effectually anſwer the purpoſe of conveying away the lightning *ſafely*, without that tendency to *increaſe* or *invite* it.

My further reaſons for diſapproving of *points*, in all caſes, where conductors are judged neceſſary, are contained in a letter addreſſed to the Marquis of Rockingham, and publiſhed in the Philoſophical Tranſactions, Vol. LIV. p. 247.

There are other reaſons alſo, which I have to offer, for rejecting points on this *particular occaſion*; and which, *were mentioned at the committee*. Thoſe I ſhall lay before the Royal Society at another opportunity, for the benefit of the publick.

Royal Society Houſe,
Auguſt 21, 1772.

Benj. Wilſon.

IX. *Obſer-*

IX. *Obſervations upon Lightning, and the Method of ſecuring Buildings from its Effects: In a Letter to Sir* Charles Frederick, *Surveyor-General of His Majeſty's Ordnance, and F. R. S. By* Benjamin Wilſon, *F. R. S. & Ac. R.* Upſ. *Soc.*

SIR,

YOUR ſtation, as Surveyor-General of His Majeſty's Ordnance, being ſuch, as makes the ſubject of this paper particulary intereſting to you, I preſume an apology for this addreſs will be wholly unneceſſary.

Upon an application of the Board of Ordnance to the Royal Society, in July laſt, a committee was appointed, to conſider of the propereſt method for ſecuring the *Magazine* at *Purfleet* from miſchief by lightning: which committee reported to the council of that learned body, what they thought neceſſary to be done upon that occaſion. The council, afterwards, tranſmitted to the board, a copy of that report, together with another paper written by myſelf, in conſequence thereof.

For, during the conſideration of that buſineſs, ſome doubts having ariſen in my mind, with regard to the propriety of *points*, which were propoſed to terminate the top of each conductor; and thoſe doubts being founded upon experiments and obſervations, I could not conſiſtently ſubſcribe to that report, nor ſuppreſs my opinion, on a ſubject of ſuch importance.

Whatever may be the ſentiments of others reſpecting thoſe doubts, yet, they being the reſult of my mature conſideration, I thought it my duty to propoſe them to the committee; and further to expreſs my diſſent, in writing, to that particular part of their report: giving, at the ſame time, ſome of the principal reaſons for ſuch diſſent; and referring them, for further ſatisfaction on this ſubject, to a letter which is already publiſhed in the Tranſactions of the Royal Society.

As that diſſent is the origin of this paper, a copy of it is here inſerted. See page 48 of this Volume.

AGREEABLE to the declaration at the end of the above diſſent, I ſhall now proceed to offer my further reaſons for objecting to pointed conductors.

Experience, which is our beſt guide in all phyſical enquiries, but particularly in electrical ones, every day convinces me, that we know but little of that ſubtile fluid, which operates ſo *ſecretly*, and at the ſame time ſo powerfully, upon the earth, and its atmoſphere I confeſs that I am even now leſs acquainted with the principle of its action, than I thought I was twenty years ago: the *ſmalleſt diff-*

ferences

ferences in the circumſtances of our experiments, frequently cauſing very material differences in their reſults. And perhaps no one, who has not applied his mind cloſely to enquiries of this kind, could conceive how the *pointing* a piece of metal, or *not*, ſhould make any material difference in the experiment.

The electrician has it always in his power to convince any one of the fact, who, through inexperience, may be inclined to entertain the leaſt ſcruple about it: for *even from thoſe experiments* to which it was thought proper to appeal at the committee, it appeared, that the difference in the effects upon this fluid, between *pointed* and *blunted* metal, is as 12 to 1.

A thunder-cloud therefore, according to that reaſoning, (the circumſtances of it being ſuppoſed to be nearly ſimilar with what is called the prime-conductor in thoſe experiments), if it acted at 1200 yards diſtance upon a point, would require a blunted end to be brought within the diſtance of 100 yards; and beyond thoſe limits, would paſs over it, without affecting it at all. On this occaſion permit me to obſerve, that the *longer* the conductors are above any building, the *more danger* is to be apprehended from them; as they will in that caſe approximate nearer in their effects to thoſe that are pointed. And that is one reaſon why I was not for adviſing the propoſed conductors at *Purfleet*, to be ſo high as *ten feet* above the magazines, and more particularly upon that building called the *Board-houſe*, which ſtands conſiderably higher than the magazines themſelves.

But, before we advance farther into this ſubject, it may be proper to ſhew the reaſons for introducing a

pointed apparatus, when the experiment upon lightning was firſt propoſed: what good conſequences were derived from that experiment: and why, upon further experiments and obſervations, ſuch points ought now to be laid aſide, when our intention is *not to make electrical experiments*, but by the means of conductors, *to preſerve buildings from the dangerous effects of lightning*.

Dr. Franklin, in his conjectures, that lightning and electricity were one and the ſame fluid, conſidered how he ſhould *invite*, or *bring down and collect the lightning*, ſo as to make experiments upon it.

And he concluded, from obſervation, that the likelieſt method would be, to make uſe of ſuch an apparatus for the purpoſe, as was moſt ſuſceptible of electric effects; or, in other words, ſuch an apparatus as would receive the electric fluid with the greateſt eaſe.

Repeated experiments taught him, that *metals* had the property of receiving that fluid, with more eaſe than other ſubſtances.

He alſo learnt, from the like experience, that metals *by being pointed*, were rendered ſtill more ſuſceptible of receiving it.

And therefore, he propoſed an experiment to be tried, " Whether it was not in our power to invite, " or bring down the lightning, by an apparatus, " conſiſting of an *electric ſtand*, and an iron rod, " twenty or thirty feet in length, riſing upright " from the middle of the ſtand, and at the top, " terminating in a very *ſharp point*." This apparatus was recommended to be put upon ſome high building,

building, with the expectation, that if a thunder-cloud ſhould happen to paſs near this apparatus, ſome quantity of the lightning depoſited therein would probably be collected in the rod, by means of the very ſharp point, and the electrical ſtand at the foot of the rod

That this contrivance anſwered the end he firſt propoſed, we have had ſufficient evidence.

And it is no wonder if, after this great diſcovery, we find him, and other electricians, purſuing new experiments of this kind, and raiſing thoſe points higher into the air, to collect ſtill greater quantities of that fluid which occaſions lightning. Nor need we be ſurprized, after knowing that lightning could be brought down from the heavens by ſo ſimple an apparatus, and after experiencing its ſubtile effects to be ſimilar with the electric fluid, that the Americans, and others, upon Dr. Franklin's recommendation, adopted the principle of ſecuring their buildings from its dangerous effects, by raiſing above their houſes rods of iron, very *ſharply pointed*, and applying *wires* from the ends of thoſe rods, down the outſide of their houſes, to the ground.

But though there appeared many arguments at that time in favour of ſuch conductors, yet experiments and obſervations, at laſt, induced Dr. Franklin to *alter his opinion* in reſpect to thoſe *wires*, and to ſubſtitute in their place *rods of iron:* ſtill retaining the principle of having the rods at the top *ſharply pointed*; and many of the Americans, as well as Europeans, approved of the alteration, as appeared afterwards, from conſtructing their conductors accordingly.

About that time great attention was given, and many new experiments were made, in conſequence of the frequent dangerous effects, which lightning was obſerved to produce in ſome valuable buildings, by rending and daſhing to pieces very large ſtones and timbers, which were connected together by cramps and bars of iron: and at other times breaking and melting part of thoſe rods, and ſometimes *exploding wires*, even of a conſiderable thickneſs, like ſo much gunpowder.

From careful obſervations of theſe extraordinary appearances produced by violent ſhocks of lightning; and upon making other experiments relating to a certain reſiſting power *in*, or *upon*, all bodies, which appears to act againſt the attacks of lightning, as well as againſt the electric fluid, philoſophers were enabled to aſſign the reaſon, and, it is apprehended, upon a ſolid foundation, why Conductors ſhould be made of *metal*, in preference to all other materials; as the power of reſiſting ſuch attacks is leſs in metals than in wood, ſtone, or marble.

And that this reſiſtance might be the more ſimple and uniform, it appeared the moſt eligible to have the conductors made of *one continued piece of metal only*, and of *an equal diameter throughout*. But what that diameter ought to be, depended upon other circumſtances, ſome of which are taken notice of in a former paper, referred to above, which I laid before the Royal Society.

By this hiſtorical ſketch, we ſee the propriety of Dr. Franklin's introducing points, and the advantage philoſophy has derived from them: by aſcertaining

that lightning and electricity are one and the ſame fluid: which appears to be diffuſed every where, at leaſt upon *this earth* and in *the atmoſphere*.

But when *curioſity*, which I apprehend was one of the firſt motives for introducing points to invite the lightning, was *ſatisfied*; and *experience* had taught us, that we had it in our *power* to *collect* that fluid which occaſions it: and when the *principle* of its action was from experiments thus inveſtigated and *aſcertained*, this manner of *invitation*, viz. by uſing points, ought, in my opinion, to have *ceaſed* *; becauſe a greater quantity of lightning, than we have yet experienced, may chance to attack us.

For we are ſo far from knowing how great the magazine of lightning may be in the heavens, or in the earth, when it is ready to diſcharge itſelf, either by one or more exploſions, that we are ignorant, *even of the quantity actually diſcharged*, whenever any ſtroke from lightning viſits us.

Nor can the ableſt philoſopher *fix the limits* of the *greateſt diſcharge* that may poſſibly happen.

Seeing then how vain it is to look for any thing like *abſolute ſecurity*, in all caſes, it ſurely behoves us to proceed with *caution*. And it is for that reaſon I have always conſidered pointed conductors as being *unſafe*, by their great readineſs to *collect the lightning in too powerful a manner*. And leſt the conductors without ſuch points, ſhould be too ſlender for very

* Unleſs where the electrician, like *Profeſſor Richmann* (who was killed by it) at his *own hazard*, chuſes to make further obſervations on lightning.

violent

violent attacks, in places of great conſequence, I have always recommended the having them above *four times* larger in diameter, than what are commonly made uſe of, to the end our ſecurity may be the greater, by opening a larger paſſage for any extraordinary diſcharge, and ſo far leſſening the danger to be apprehended from it.

I ought not, in this place, to omit taking notice of a paper, containing ſome further experiments and obſervations, which were produced at the committee, to ſhew, among other things, that pointed metals were more diſpoſed to receive the lightning, by virtue of a *repelling principle* in the lightning, as well as the electric fluid, which acted upon the *natural quantity* of the fluid contained within the metal, at a conſiderable diſtance from the *point*, cauſing, if I may be allowed the expreſſion, a kind of *vacuum* therein; but I ſuppoſe the author means to a certain diſtance only.

So far from diſputing this philoſophy, I readily admit the fact.

But, I am afraid, *every attempt* to prove that pointed conductors may be ſo diſpoſed to receive this fluid more readily, will not mend the argument in the leaſt; becauſe, the more we leſſen the power of reſiſting, even ſuppoſing the whole Conductor to be in that ſtate, the more we increaſe the power of invitation.

In regard to other experiments, with *locks of cotton* *, which are acted upon in a particular manner

* Dr. Franklin's Experiments.

by

by the appoſition of points, and the concluſions drawn from thence, in favour of pointed conductors, as cauſing ſimilar effects upon the *fragments or ſmall clouds*, which, hanging below the thunder-clouds, have been ſuppoſed a kind of *ſtepping-ſtones*, for the lightning to paſs upon, towards the earth: ſuch pointed conductors being ſuppoſed to occaſion thoſe *fragments* to *retire up into the cloud* from whence they were ſuſpended; and on that account, to prevent a ſtroke from lightning, which might otherwiſe have happened, I ſhall, for the preſent, wave entering into this philoſophy, as I could wiſh the conjecture to be reconſidered; becauſe I apprehend it is liable to many objections, which to enumerate would carry me beyond the proper bounds of ſuch a paper as this. However, if the ſame opinion ſhould again be offered, and brought in argument, it may be worth while to enter more *deeply* into the enquiry.

If thoſe gentlemen, who argued at the committee for the *neceſſity of points*, could have made it appear, that ſuch points draw off, and conduct away, the lightning *imperceptibly* and *by degrees*, *without cauſing any exploſion*, during a thunder ſtorm (which ſeems to have been once the opinion of Dr. Franklin) I ſhould readily have ſubſcribed to their Report.

But experience ſhews us, that the fact is otherwiſe: there being many inſtances, where violent exploſions of lightning have happened to *conductors* that were *ſharply pointed*. And *three* in particular, the accounts of which are inſerted in a publication of Dr. Franklin's *, where the *points*

* Dr. Franklin's Experiments, p. 394. 416, 417, &c.

were *diſſipated*, or *deſtroyed*; and a ſmall part of an iron rod melted next the points of one of them; and alſo at the ſeveral crooked ends of the rods below, where they were hooked on to each other, and formed the conductor belonging to Mr. Maine in North America. But as thoſe letters are long, and contain ſeveral other curious facts, I ſhall reſerve them, together with ſome further obſervations upon the *nature* and *power* of that *reſiſting principle*, which is found to act ſo ſenſibly againſt the attacks of the electric fluid, or lightning, to ſome future diſſertation.

There is no building, that I know of, more expoſed to this kind of danger, than the *Eddyſtone Lighthouſe*, as it ſtands upon a rock in the ſea, ſeveral miles from land. The fixing of a conductor to that building, was thought highly proper; and the fixing of a *point* upon it, as highly improper. It was therefore reſolved upon to put up a *conductor without a point*, that no more lightning might be unneceſſarily ſollicited to the building, and that all the lightening, which accidentally fell on it, might be conveyed away without injuring it. This conductor was *fixed* twelve years ago, and the building has ſince received no injury from lightning *.

There is another edifice of great conſequence, I mean St. *Paul's Church*, which ſtands much expoſed, from its heighth, to accidents by lightning. The dean and chapter of that cathedral, thought it an object deſerving the ſerious attention

* N. B. A former building erected for the ſame purpoſe, upon this rock, was ſet on fire by lightning.

of the *Royal Society*. A committee was therefore appointed, in conſequence of their application: and proper *conductors* were put up, in the ſeveral places where they were thought neceſſary, from the top of the *lanthorn* to the *ſewers* underground. And notwithſtanding particular care was taken, to have the additional metal either of a conſiderable diameter, or an equal quantity of it formed into other ſhapes, for the conveniency of the ſeveral places; yet part of thoſe conductors, conſiſting of iron, in the *ſtone gallery*, ſhewed marks of their having been made *conſiderably hot*, if not abſolutely *red*, by a ſtroke of lightning which happened in March laſt (as appears by a letter which I communicated to the Royal Society from one of the vergers of that church, Mr. Richard Gould) who had examined the conductors the morning following, along with Mr. Burton of the ſame cathedral *, and that the appearances were in general,

* Mr. Gould acquaints us in his letter, that he examined the four conductors in the lanthorn and ſtone gallery of St. Paul's Church, the morning after the lightning happened. That no marks whatſoever appeared upon the conductor to the *ſouth*, which was the firſt he attended to. That he examined next the conductor to the *Weſt*, and obſerved a *thick ruſt* lying upon the pavement in the ſtone gallery, as if it had been cleaned off, from the conductor, with a tool: that ſeveral parts of the iron appeared *black*, particularly the ſcrews or nuts: ſomething like the effects left by gun-powder upon iron or ſteel, or a ſmoaky fire.

That the conductor to the *North*, ſhewed no marks, no more than that to the South.

But that upon examining the conductor to the *Eaſt*, he found ſtronger marks *abundantly*, than on the *Weſt* conductor,

ral, as the verger's letter related them to me. Mr. Delaval and myſelf attended, about a week afterwards, to obſerve them, and their particular

it being *much blacker*; particularly on the nuts and ſcrews: the ruſt lying in greater quantities on the pavement. *And the extreme part of the conductor that goes into the water trunk*, ſeemed like a piece of iron *newly taken out of a forge* by a ſmith, without working it on the anvil.

N. B. Mr. Gould has ſince added to the account in his letter, ſome circumſtances which I apprehend ought not to be omitted. He ſays, that where the end of the conductor, on the Eaſt-ſide, points towards the water trunk, a ſtone ſurrounds part of it, leaving an interval, half an inch wide or more, between them, and about four or five inches long, which is a little more than the breadth of the conductor. That this interval was *filled up with dirt*, and had been ſo for ſome time, occaſioned by frequent ſhowers of rain waſhing the pavement in the ſtone-gallery. That, after the lightning happened, he obſerved a *hole* was made through the *dirt*, *one quarter of an inch in diameter*, and about *two inches in length*. That the hole was cloſe to the iron; and that, upon ſtooping down his head, he perceived a very diſagreeable *ſmell of ſulphur* from the ſtone, dirt, and conductor, particularly the laſt.

Upon hearing this account, Mr. Delaval and myſelf, a few days ago, went and examined the conductors again; but more carefully than before. For, upon cauſing the ſtone to be removed, which covered the top of the water trunk, we had an opportunity of examining near two feet more of the iron which points to the water trunk, than we could perceive before this ſtone was removed. When we obſerved, that the conducting iron did not touch the lead. We likewiſe obſerved, that there was a *very thick coat of ruſt* all over that part of the iron; *particularly at the end next the lead*, where the water entered the trunk.

As the neceſſity of attending to theſe circumſtances will be obvious to any one, who is but in the leaſt degree acquainted with theſe reſearches, *the danger of neglecting them will be ſeen in the ſtrongeſt light, by the gentlemen of the committee* who recommended the conductors for the ſecurity of that Cathedral.

ſituations,

ſituations ; with the circumſtances attending them, when we were very well ſatisfied with his account, notwithſtanding it had rained in the *interim* for three days together.

It is worthy of note, that thoſe conductors did *not terminate in a point*, nor was any *point put* upon the *croſs at the top*.—And yet Dr. Franklin was of that committee.

If points are ſo eſſential to our ſafety, why was not the reaſon enforced at the committee, for having them on that capital edifice? For my part, I think it was a happy circumſtance, that there was *no point* fixed upon the top of the church, to ſollicit a greater quantity of lightning at that moment, than what fell upon the conductors, circumſtanced as they were: as *that quantity* was great enough to *heat ſo conſiderably* a *bar of iron*, near *four inches broad*, and about *half an inch thick*.

This powerful effect, reminds me of another inſtance ſtill more extraordinary, which happened in Martinico, and is related by *Captain Dibden*, where a bar of iron, *one inch in diameter*, was by a violent ſhock of lightning *reduced in one part of it, to the thickneſs of a ſlender wire only*. See Ph. Tr. Vol. LIV. p. 251.

Since then we are at all times ignorant of the quantity of *lightning in the* earth and its atmoſphere; and the difference in the effects, between blunted and pointed, ends, in cauſing a diſcharge in our electrical experiments, appears to be as *one to twelve*; it is eaſy to comprehend the very great danger this noble fabrick has probably eſcaped, by having no pointed apparatus upon it.

From

From the above obſervations, I am naturally led to conſider a part of the proceedings of the committee, reſpecting the *magazines* at *Purfleet*; when a certain number of conductors, with tapering points at the top, were reſolved upon, as neceſſary, to protect the ſeveral buildings where the powder is depoſited. For it was *agreed* upon at the ſame meeting, that the *Board-houſe*, which is a large building for the uſe of the board-officers, and which ſtands conſiderably higher than the magazines, as was obſerved above, *did not require any point at the top:* becauſe it was apprehended to be *perfectly ſecure*, by reaſon of the copeings on the roof, the gutters and pipes to carry off the water, being all of lead: and further, becauſe thoſe pipes communicated with two wells, which always contained water.

I was not a little ſurprized at this laſt reſolution, which appeared to be ſo inconſiſtent with the former. Becauſe, if points were neceſſary in one place, they ought to be ſo in another. And on the other hand, if the Board-houſe is ſecure by the leaden accidental conductors, which have no points, why ought not the magazines to be equally ſecure, when put into the ſame circumſtances?

I therefore enforced the *inconſiſtency* of ſuch a reſolution in the ſtrongeſt terms. Notwithſtanding which, the gentlemen, at that time, thought proper to confirm their reſolution. However, at the next meeting of the committee, I obſerved that they had been pleaſed, in the mean time, to make an amendment in favour of points for the board-houſe;

houſe; which amendment was no ſooner propoſed, than approved of.

Why my obſervation was rejected at the preceding meeting, I muſt leave to the judgment of others. But it certainly carries an appearance, as if *manifeſt contradiction*, upon further reflection, muſt have been the *cauſe* of that alteration.

And I am inclined to believe, from ſome gentlemen of the committee expreſſing their opinion, "*of its being a matter of mere indifference whether "blunted or pointed conductors were made uſe of,*" that they have not conſidered this ſubject, with all the due attention which ſo important an object deſerves.

For if our experiments ſhew, that *points*, from the *nature* of their *ſhape*, and *other* circumſtances attending them, reſiſt the attacks of this fluid leſs than blunted ones; and that blunted conductors, of proper dimenſions, are ſufficient to convey away the lightning *ſafely*, whenever it attacks them; why ſhould we have recourſe to a method, which is at beſt uncertain; and which ſome time or other may be productive of the moſt fatal effects?

But perhaps no argument can be brought with more force againſt the principle of points, than Dr. Franklin's own words, which are publiſhed in his experiments, p. 481, where he declares *poſitively*. "Buildings, that have their roofs covered with "lead, or other metal, and ſpouts of metal conti-"nued from the roof into the ground to carry off "the water, *are never hurt by lightning*; as when-"ever it falls on ſuch a building, it paſſes in the "metals, and not in the walls."

This

This is the caſe with the *Britiſh Muſeum*, a building alſo of conſiderable conſequence, where there are no other *conductors*, than what are formed by the *copeings*, *gutters and pipes*, which are all of lead, and communicate with the ground. Now it is from the great quantity of metal contained in the ſeveral pipes, together with the other circumſtances attending them, that I conſidered that building (in a former paper laid before the royal ſociety) as being ſufficiently ſecured, from thoſe dangerous accidents.

But if any gentleman ſhould be diſpoſed to entertain a doubt about it, or indeed of any other part of my reaſoning on this ſubject, a *declaration* of thoſe doubts may be attended with good conſequences, as they will neceſſarily open the door to a more minute inveſtigation.

I have now, Sir, gone through the reaſons which I propoſed to lay before the royal ſociety for the rejecting of points. And I am very ſorry, in the courſe of this letter, to have been under the neceſſity of mentioning any differences in opinion, which paſſed between the members of the committee, to whom this important matter was referred. I think, however, I ſhall ſtand excuſed to the ſociety, and the public, when it appears, as I hope it now ſufficiently does, what my motive has been; namely, to ſtate clearly, and impartially, the objections which I conceived to lye againſt pointed conductors: and to diſcloſe *without any reſerve*, the principles on which ſuch objections are grounded.

I am, S I R, with the greateſt reſpect,
Your moſt obedient, and moſt humble ſervant,

8th December, 1772,
Great Ruſſel-ſtreet,
Bloomſbury.

Benjamin Wilſon.

P. S.

P. S. Mr. Delaval, who was one of the committee, has given me leave to inſert his opinion upon this ſubject; which is this. That he concurs with me in thinking that ſuch conductors as are elevated higher than the buildings to which they are applied, or are pointed at the top, are *improper* and *dangerous*.

He was deſirous of delivering his opinion at the committee: but, as the meetings of it were held in the ſummer only, his abſence from London prevented his attendance.

X. *A Letter to Sir* John Pringle, *Bart. Pr. R. S. on pointed Conductors.*

SIR,

Read Dec. 17, 1772. HAVING heard and considered the objections to our Report, concerning the fixing pointed Conductors to the Magazines at Purfleet, contained in a letter from Mr. Wilſon to Sir Charles Frederick, and read to the Royal Society, we do hereby acquaint you, that we find no reaſon to change our opinion, or vary from that Report.

We have the honour to be,

SIR,

Your moſt obedient,

humble ſervants,

December 17, 1772.

H. Cavendiſh,
W. Watſon,
B. Franklin,
J. Robertſon.

XI. *Aſtro-*

XI. *Aſtronomical Obſervations made at* Chiſlehurſt *in* Kent, *by the Reverend* Francis Wollaſton, *F. R. S.*

Received November 12, 1772.

Read Dec. 17, 1772.

I the laſt year did myſelf the honour to deliver to this Society, an account of the going of an aſtronomical clock with a wooden pendulum, for the year preceding; together with ſuch obſervations as I had made in this place; the latitude of which is 51° 24′ 33″ North, and its longitude 4′ 39″ = 18″,6 in time, Eaſt of the Royal Obſervatory at Greenwich. As that account ſeemed not wholly unacceptable, I have now continued it down to the preſent time: omitting however the ſeries of tranſits; which I gave in the former paper, merely to ſhew the exactneſs with which they might be taken with a very ſmall inſtrument, beyond what I had expected to find.

The rate of the clock deduced from the obſervations of this laſt year, will not be found ſo uniform as the foregoing. To what cauſe to aſcribe it, I am not certain. I think not to heat: perhaps to the great drought of the ſummer. However, its acceleration or retardation was not deſultory, but ſufficient

ficient to be depended upon for any intermediate time. The clock was cleaned in November; and when ſet up again, loſt, between the 18th and 28th, at the rate of 7″,8 per day. The regulator was then altered, and clock ſet, and from that time never meddled with.

My obſervatory (if it deſerves that name) is a room up two pair of ſtairs, in a ſquare compact brick houſe, pretty much expoſed indeed to wind, from our high ſituation. The face of the clock turns due Eaſt. I have ſet down the *throwing-out* of the pendulum, as judged by the eye, on a ſcale behind it divided only to every 10 minutes; and therefore ſome of the variations may only be errors in judgment. How far a clock ſo ſituated may be affected by any motion of the houſe in windy weather, deſerves conſideration. I do not however perceive that it *is*, either in the *rate* or *throwing-out*.

1771.

1771.		Clock + too fast — too flow for mean ſol. time.			Gain + or Loſs —	Number of days.	Rate per day.	Throwing out on the South ſide.		North ſide.	
			′	″	″		″	o	′	o	′
Nov.	29	Clock —		0,6							
Dec.	16	—		17,9	— 17,3	17	— 1,02	1	42	1	45
	30	—		30,0	— 12,1	14	— 0,86	1	43	1	46
1772.					— 8,0	14	— 0,57	1	43	1	45
Jan.	13	—		38,0	— 6,9	22	— 0,31	1	43	1	46
Feb.	4	—		44,9	— 9,5	11	— 0,86	1	44	1	46
	15	—		54,0	— 8,9	17	— 0,52	1	42	1	44
Mar.	3	—	1	2,9	— 2,8	32	— 0,09	1	42	1	44
April	4	—	1	5.7	+ 2,0	9	+ 0,22	1	42	1	44
	13	—	1	3,7	+ 48,3	19	+ 2,54	1	38	1	4●
May	2	—		15,4	+ 42,8	11	+ 3,89	1	43	1	45
	13	+		27,4	+ 43,8	10	+ 4,38	1	42	1	44
	23	+	1	11,2	+ 41,8	9	+ 4,66	1	42	1	44
June	1	+	1	53,0	+ 61,1	14	+ 5,08	1	43	1	45
	15	+	2	54,1	+ 54,8	18	+ 6,38	1	43	1	45
July	3	+	4	48,9	+ 50,5	7	+ 7,21	1	43	1	45
	10	+	5	39,4	+ 81,1	11	+ 7,37	1	43	1	45
	21	+	7	0,5	+ 78,3	10	+ 7,83	1	43	1	45
	31	+	8	18,8	+ 58,1	8	+ 7,26	1	48	1	50
Aug.	8	+	9	16,9	+ 60,0	9	+ 6,44	1	47	1	49
	17	+	10	16,9	+ 72,9	11	+ 6.63	1	47	1	49
	28	+	11	29,8	+ 52,8	10	+ 5,28	1	44	1	47
Sept.	7	+	12	22,6	+ 31,3	8	+ 3,91	1	46	1	48
	15	+	12	53,9	+ 22,0	6	+ 3,33				
	21	+	13	15,9	+ 37,1	10	+ 3,71	1	48	1	50
Oct.	1	+	13	53,0	+ 27,6	9	+ 3.06				
	10	+	14	20,6	+ 20,0	9	+ 2,22				
	19	+	14	40,6	+ 32,4	14	+ 2,31	1	44	1	48
Nov.	2	+	15	13,0							

I have

I have this year kept a regiſter of the Thermometer and Barometer; the higheſt and loweſt ſtate of which, as I obſerved them in each month, I will here ſubjoin. I do not find that they have any reference to the rate of the clock; but they may ſerve for compariſon with other places. The Thermometer A was made by Mr. Nairne; and hangs without doors, near a window, up one pair of ſtairs, on the North ſide of my houſe; with a ſkreen to keep off the morning ſun in ſummer, but no building near it. The three firſt columns ſhew its ſtate at eight o'clock in the morning, two in the afternoon, and eleven at night. The fourth column gives the Thermometer B, in the obſervatory, near the face of the clock, at nine in the morning. The fifth ſhews the Barometer; which being portable, I cannot be certain that its ſcale is exactly placed; though I believe it is right. It is kept in a room on the ground floor.

1771.		Thermometer A. H. 8. Mat.	H. 2. P. M.	H. 11. P. M.	B. H. 9. Mat.	Barometer.
Dec.	Higheſt	52	50	53	52	30,05
	Loweſt	34	35	33	36	28,58
1772.						
Jan.	Higheſt	49	52	49	50	30,09
	Loweſt	22	30	21	29	28,63
Feb.	Higheſt	50	47	48	50	29 92
	Loweſt	25	32	24	32	28,79
Mar.	Higheſt	51	56	46	50	29,78
	Loweſt	27,5	31	27	30	28,79
April	Higheſt	51	59	50	52	30,04
	Loweſt	31	38	31	36	29,19
May	Higheſt	63	70	53	58	30,20
	Loweſt	42	47	33	44	29,35
June	Higheſt	73	82	68	73	30,12
	Loweſt	53	57	49	56	29.50
July	Higheſt	70	76,5	63	68	30,10
	Loweſt	55	61	49,5	58	29,16
Aug.	Higheſt	73	75	67	70	30,08
	Loweſt	52	62	50	60	29,23
Sept.	Higheſt	66	72	66,5	70	29,96
	Loweſt	48	58	46	45	28,82
Oct.	Higheſt	60	66	59	62	30,11
	Loweſt	44	52	43	51	28,88

Eclipſe of the Sun, ⊙ October 25, 1772.

App. time. h / //	
20 31 12	The firſt impreſſion that I perceived. But I had looked for it on the Weſtern inſtead of the Eaſtern edge, owing to a miſtake in the Almanac; ſo that the Eclipſe was now about its greateſt obſcuration. A remarkable protuberance on that part of the Moon's limb.
20 36 34	End of the Eclipſe. Obſerved with a 3½ feet Achromatic Teleſcope, magnifying 150 times.

Occul-

Occultations of Stars by the Moon. Obſerved with the ſame teleſcope, magnifying 150 times.

1772.			Apparent time.			
			h	′	″	
♄ Feb.	15.	☽ λ ♊	9	24	8	Im. good.
			10	21	16	Em. certain to 2″.
☉ April	12.	☽ ω ♌	11	35	17	Im. The ✱ certainly began to loſe of its light 2″ before it diſappeared.
			12	1	35	Em.
♀ May	8.	☽ ✱	8	16	6	Im. Seen accidentally, as I was looking at ☽ with the magn. 100. The ✱ was near ζ ♋, but leſs N. dec.
						Em. cloudy.
♄ May	9	☽ × ♋	.	.	.	Not eclipſed.
☿	15.	☽ α ♎	1ma. 11	56	50	Im. good. Night clear, and no air.
			13	0	23	Em. doubtful to 5″ or more.
			2da. 12	2	19	Im. good.
			13	8	57	Em. good.
☽ Aug.	17.	☽ ζ ♓	1ma. 10	57	25	Im. Night clear and ſtill; but ſuch undulation on ☽'s limb, that the imm. were doubtful.
			11	52	5	Em. good.
			2da. 10	58	12	Im. very doubtful.
			11	52	59	Em. very good.
☉ Sept.	6.	☽ ρ ♐	1ma. 8	41	41	Im. good.
						Em. Rain.
☽	7.	☽ β ♑	13	9	21	Im. Hazy.
						Em. ☽ ſet.
♃ Oct.	8.	☽ ✱	9	7	46	Im. near Schikardus.
						Em. not ſeen.

Eclipſes

Eclipſes of Jupiter's ſatellites. Obſerved with the ſame teleſcope, magnifying 100 times.

1772.			App. time. h	′	″	
♄	July	11.	11	22	34	Firſt ſat. im. Good.
♄		27.	9	38	2	Firſt ſat. im. Vapours.
☿		29.	10	29	59	Second ſat. im. Cloudy and doubtful.
☽	Aug.	3.	11	32	48	Firſt ſat. im. Good.
♄		22.	9	38	35	Third ſat. em. Hazy.
☉		23.	10	35	19	Second ſat. em. Doubtful. Flying clouds.
♀		28.	8	28	14	Firſt ſat. em. Very doubtful.
☉	Sept.	27.	10	53	35	Firſt ſat. em. Doubtful.
☉	Oct.	4.	9	57	14	Third ſat. em. Good.
			12	50	41	Firſt ſat. em. Good.
♂		13.	9	17	13,5	Firſt ſat. em. Good.
♂		20.	11	14	32	Firſt ſat. em. Doubtful.

Occultations of Jupiter's ſatellites, and tranſits over his diſc, and conjunctions of the ſatellites, and appearances of his belts. Obſerved with the ſame teleſcope, magnifying 150 times.

1772.			App. time. h	′	″	
♀	Aug.	7.	10	44	40	Second ſat. on the limb of ♃, in its egreſs from his diſc.
♂		11.	10	55	50	Firſt ſat. in contact with ♃'s limb.
			11	1	40	——— Total ingreſs on the diſc. The ſhadow precedes the ſat. but very little.
☿		12.	9	30	0	Appearance of ♃'s belts, as in Tab. III. Fig. I.
			10	45	0	Southern belt compleated.
♃		13.	10	15	0	Appearance of ♃'s belts, as in Fig. II.
♀		21.	9	13	8	Conjunction of firſt and ſecond ſat.
♄		29.	9	2	0	Third ſat. ſeemingly in contact with the limb.
			9	13	0	——— Occult.
☉		30	9	45	0	Second ſat. partly covered by the limb.
			9	48	40	——— Occult.

1772.		App. time. h	′	″	
☽ Sept.	7.	9	38	0	Appearance of ♅, with the ſhadow of the fourth ſat. central. The ſat. itſelf not viſible. Fig. III.
		11	10	0	The Southern belt equally advanced with the ſhadow.
♄	12.	8	39	20	Appearance of ♅, and ſhadow of the firſt ſat. central, Fig. IV. The ſhadow much ſmaller than that of the fourth.
		9	24	30	Southern belt advanced half way.
		9	29	0	Sat. now off the diſc, and ſhadow ſtill on; about equidiſtant from the limb.
		9	40	46	Shadow going off, in contact with the limb.
♂	15.	7	27	0	Appearance of ♅, as in Fig. V.
		8	22	20	Southern belt compleat, and dark ſpot (α) in the large belt, central.
		8	27	20	Second ſat. on the limb.
		8	30	20	——— Total ingreſs on the diſc.
		10	41	0	Shadow advanced about $\frac{1}{3}$ on the diſc: not very viſible: preceded by another dark round ſpot, ſmaller; I know not what. Fig. VI.
☿	16.	8	36	0	Shadow of the third ſat. about its own diameter from ♅'s limb. Well defined.
		9	19	0	Third ſat. quite off the diſc. The ſhadow was then advanced about $\frac{1}{3}$.
		9	33	0	Southern belt reduced to about $\frac{1}{3}$. The ſhadow advanced about $\frac{2}{3}$. Fig. VII.
		9	49	0	Shadow central.
		11	2	30	Shadow advanced about $\frac{4}{5}$. Southern belt then ſcarce viſible on the Eaſt ſide.
		11	35	0	Shadow either gone off, or not viſible on the limb. Southern belt now come on $\frac{1}{4}$ or more. Vide Fig. VIII.
♃	19.	8	43	30	Firſt ſat. ſeemingly in contact with ♅'s limb.
		8	52	0	——— Total ingreſs.
		9	7	40	Conjunction of ſecond and third ſat.
		9	32	0	Shadow of firſt ſat. half entered.
		9	35	30	——————— totally entered.
		10	58	0	——————— advanced about $\frac{2}{3}$.
		1	1	0	Firſt ſat. on the limb.
		11	4	30	——— external contact. Vide Fig. IX. Shadow very ſmall, but well defined.

1772.	App. time.	
	h ′ ″	
☉ Sept. 27.	7 37 30	First sat. in contact.
	7 42 0	——— occult.
♃ Oct. 1.	8 9 26	Conjunction of third and fourth sat.
	8 14 45	Second sat. in contact.
	8 20 0 ::	——— occult.
☉ 4.	9 27 40	First sat. in contact.
	9 32 30	——— occult.
☽ 5.	8 29 45	First sat. in contact, just gone off the limb. The shadow at that time about $\frac{2}{3}$.
♂ 20.	7 40 15	First sat. in contact.
	7 44 20	——— occult.

These last observations are not to be depended upon, within several seconds; and yet, if pursued regularly, might have their use. I was led into making them from what M. Messier transmitted to this Society, in a former paper, Vol. LIX. N° LXIV. Indeed the hint he there gave, concerning the superiority of achromatic telescopes above all others for these purposes, induced me to procure one from Mr. Dollond, of his last construction, with a triple object-glass; which, I believe, is excellent in its kind: at least, it has fully answered the highest expectation I had formed of them.

The drawings shew how Jupiter appeared to me at different times. The broad belt across the middle, I cannot tell what to make of; sometimes it appears as a combination of several smaller ones; but usually clouded, or waved, in the middle, with its edges in general darkest; perhaps from their opposition to the contiguous parts, which are always much brighter than the rest of the planet. There have been two places in that belt particularly dark (especially that

marked α, Fig. V.), which I have ſeen latterly, whenever that ſide of γ has been turned towards us; though I did not perceive them Auguſt 12, when I took Fig. I. The Northern belt, I believe, is continued uniformly round the planet; the Southern one reaches little above half way. They are both of them darker, I think, than the general complexion of the largeſt. There is beyond each of theſe, a light part, not equally bright with thoſe zones near the middle; and from thence a gradual ſhading off to the poles themſelves; particularly to the North. The drawings are, as the objects appear in an aſtronomical teleſcope, and therefore inverted.

Chiſlehurſt,
November 10, 1772.

Francis Wollaſton.

THE

THE Aſtronomer Royal, who was preſent at the reading of this paper, expreſſed a wiſh that a particular account of the mechaniſm of the clock itſelf, might accompany the regiſter of its going; as no conſideration ought to be omitted which might ſerve to elucidate theſe matters.

My clock is in general of the plain kind, moving in braſs pivot-holes, beating dead ſeconds, with the common ſteel paletts; but of good workmanſhip, as being made by Mr. Holmes. The rod of the pendulum is of deal, as mentioned in a former paper; to which the ball itſelf (weighing about fourteen pounds) is ſcrewed faſt, there being a ſmaller weight underneath for a regulator. Its ſuſpenſion is ſomewhat particular. The ſpring A is not hung in a ſlit as is uſual; but is faſtened to a tranſverſe piece B, on which it reſts upon the ſides of the cock. The ſhoulders of this piece confine the pendulum from any lateral motion, as much as it would be in a ſlit; but it is at full liberty to hang perpendicularly under its point of ſuſpenſion, without any ſtrain on either edge of the ſpring above the other. The crutch is alſo of an unuſual make. The bottom of the ſtem, inſtead of receiving the crutch-pin, is turned ſideways; at right angles to itſelf, but parallel to the back-plate. This piece D reaches about an inch, and at the end has a ſmooth ſteady joint E (known to the workmen by the name of a ſocket and ſtud), whoſe axis lies horizontal. From that joint there is a return F, of equal length with the former piece; at the end of which is the crutch-pin G, nearly coinciding with the end of the ſtem.

This return together with the crutch-pin, if the pendulum were removed, would fall down as repreſented by the dotted lines *f*, *g*. The deſign of this mechaniſm is that any friction at the crutch-pin, which in the common conſtruction is a ſmall ſliding up and down at every oſcillation, is hereby converted into a ſmaller circular motion in a ſmooth pivot-hole. Both theſe, I underſtand, are contrivances of the late Mr. Henry Hindley of York: and may be ſeen in the regulator at Mr. Holmes's in the Strand, which is conſtructed in the ſame manner. The cock is faſtened to the back-plate of the clock itſelf; but the clock-caſe is made ſtronger than uſual, and is firmly ſcrewed to the wall, independent on the floor.

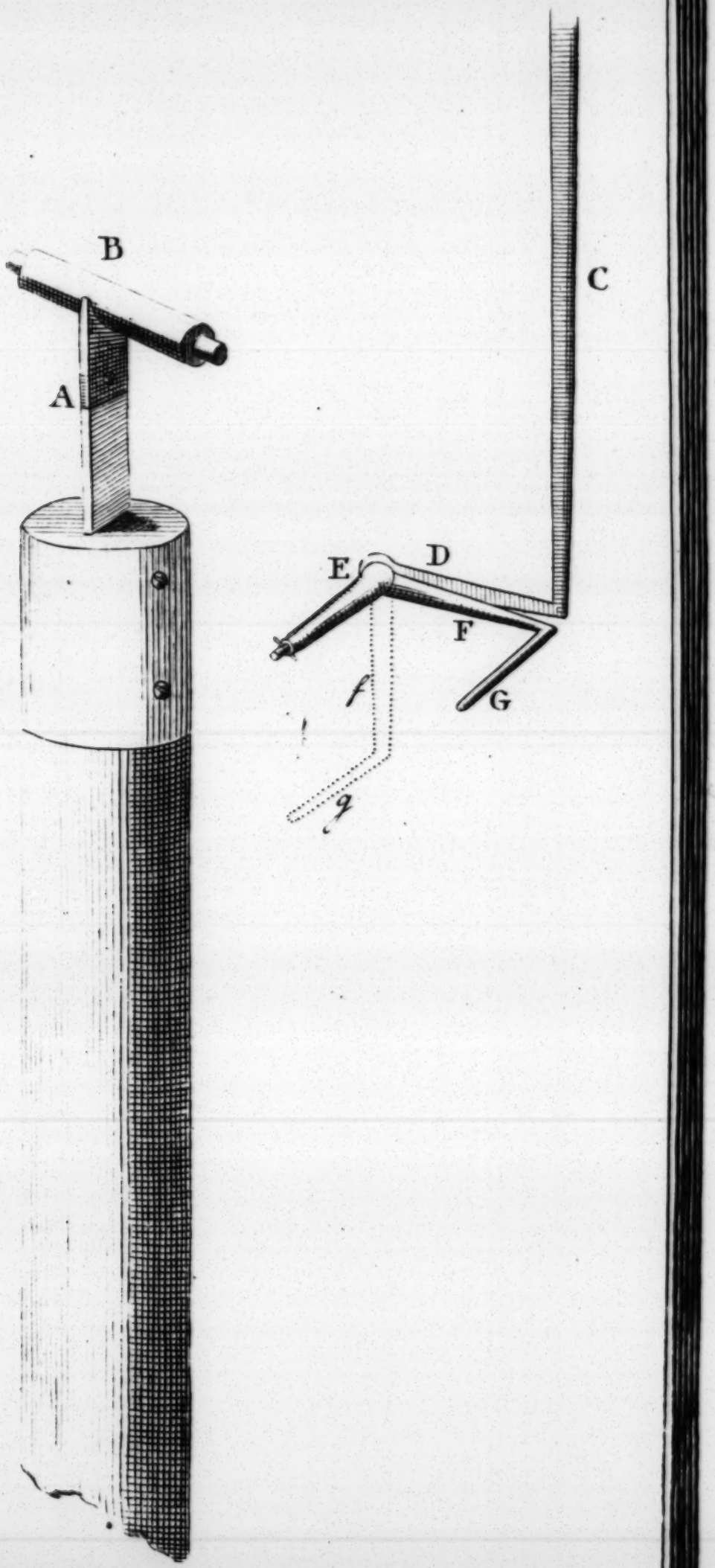
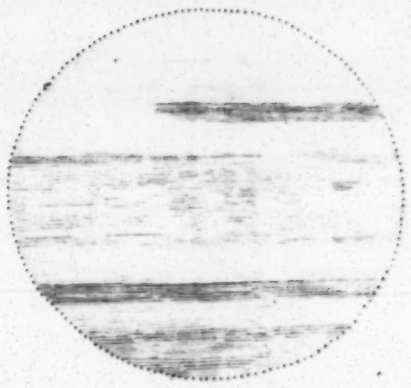
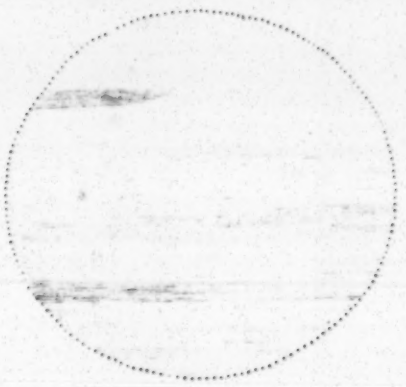

B
A
C
E
D
F
f
G
g

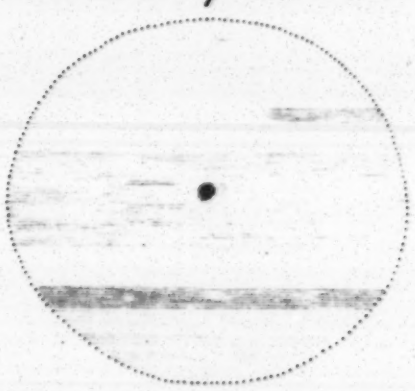
Fig. I.
Aug. 12. 1772
h. 9. 30. 0.



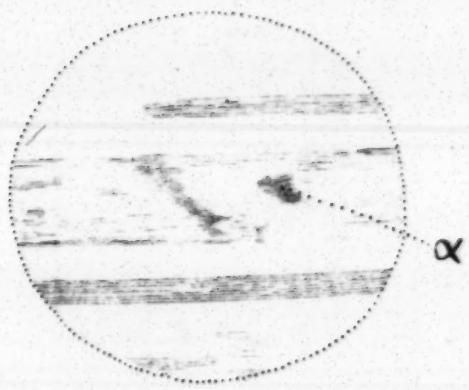
II.
Aug. 13
h. 10. 15. 0.



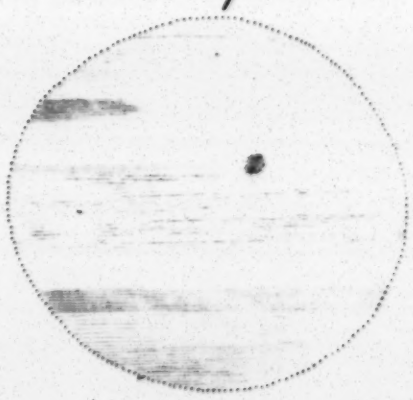
IV.
Sept. 12. h. 8. 39. 20
Shadow of 1st Sat.



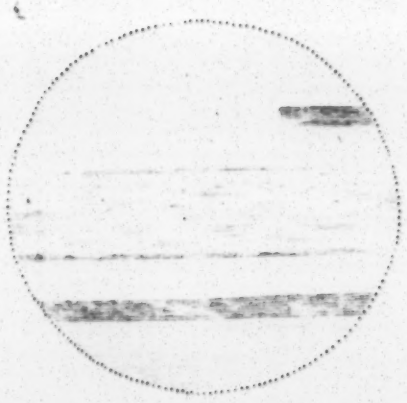
V.
Sept. 15. h. 7. 27. 0.



VII.
Sept. 16. h. 9. 33. 0
Shadow of 3d Sat.

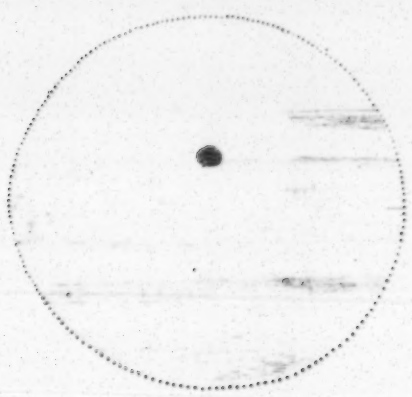


VIII.
Sept. 16. h. 11. 35. 0.



J. Basire Sculp.

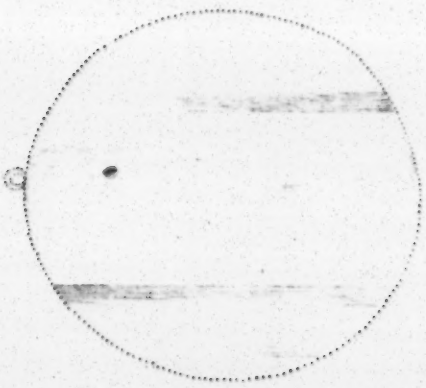
III.
Sept. 7. h. 9. 38. 0.
Shadow of 4th Sat.

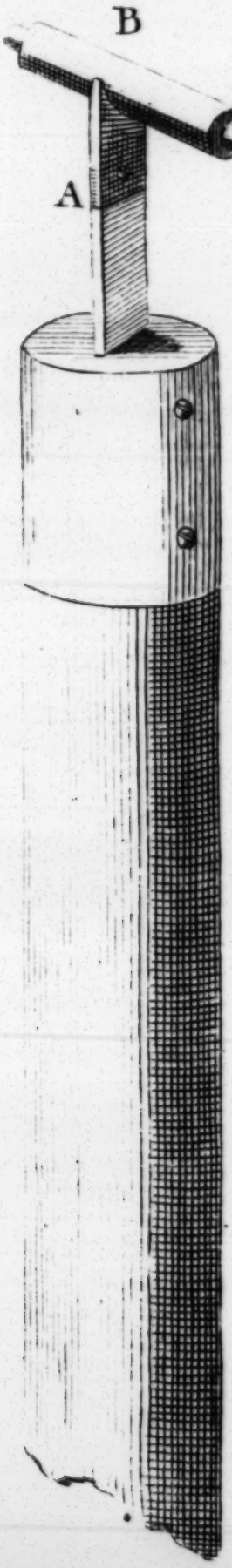


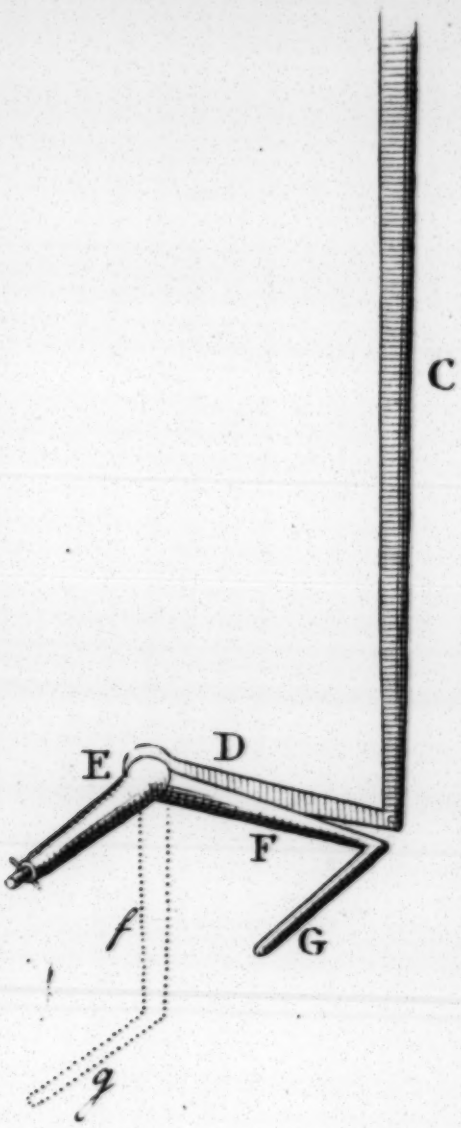
VI.
Sept. 15. h. 10. 41. 20
Shadow of 2d. Sat.



IX.
Sept. 19. h. 11. 4. 30
1re Sat. & its Shadow.







XII. *A Letter from Dr.* Ducarel, *F. R. S. and F. S. A. to* William Watſon, *M. D. F. R. S. upon the early Cultivation of Botany in* England; *and ſome Particulars about* John Tradeſcant, *a great Promoter of that Science, as well as Natural Hiſtory, in the laſt Century, and Gardener to King* Charles I.

SIR,

Read Nov. 2, 1772. YOUR love for Botany, and your great knowledge in that ſcience, will, I hope, be a ſufficient apology for laying before you ſome obſervations which have occurred to me on that ſubject. And I flatter myſelf that the following anecdotes will not be altogether unacceptable to you.

The ſciences, we know, are ſubject to revolutions. But is it not a very extraordinary one that Botany, ſo uſeful to mankind, and ſo well known to the ancients, ſhould, for ſome ages, abandon Europe, and remain almoſt unknown there till the ſixteenth century; when it is ſuppoſed to have ſuddenly revived; and has ſince, by the induſtry of the moderns, been brought to the higheſt perfection?

The truth, however, is, that Botany returned into England long before this æra. It was brought back here by the Saxons; ſince whoſe time, I ſhall endeavour to ſhew, that it hath always flouriſhed, more or leſs, in this kingdom.

I found my opinion upon the authority of the four following Saxon manuſcripts.

Two in the Bodleian Library, viz.

(*a*) N° 4125. Herbarium Saxonicum.
(*b*) N° 5169. Liber Medicinalis MS. continens Virtutes Herbarum Saxonicè.

And two others in the Harleian Library, viz.

N° 5066. entitled, Herbarium Saxonicè.
N° 585. Tractatus qui ab Anglo-Saxonibus dicebatur LIBER MEDICINALIS: ſcil. L. Apuleii Madaurencis Libri de Virtutibus Herbarum, Verſio Anglo-Saxonica.

This Lucius Apuleius of Medaura was a famous Platonic philoſopher, who flouriſhed about A. D. 200.

From this time I have met with no MS. concerning Botany, till the thirteenth century, when (*c*) Biſhop Tanner mentions three MSS. on this ſubject, written by Gilebertus Legleus, ſive Anglicus, a phy-

(*a*) Cat. MSS. Angliæ, p. 185.
(*b*) Ibid. p. 562.
(*c*) Bibliotheca, p. 474.

ſician,

ſician, who flouriſhed in the year 1210, entitled,

1. De Virtutibus Herbarum, MS. Bodl. Digb. 75.
2. Gilberti Liber de Viribus & Medicinis Herbarum, Arborum, & Specierum, MS. olim Monaſt. Sion.
3. De Re Herbaria, Lib. I.

(*d*) The Biſhop likewiſe mentions one John Ardern, a famous ſurgeon, who lived at Newark in Nottinghamſhire from 1349 to 1370, as the author of a MS. (now extant in Sir Hans Sloane's library), entitled, Volumen Miſcellaneorum de Re Herbaria, Phyſica, & Chirurgica.

In the Aſhmolean Library are the following MSS. viz.

(*e*) (N° 7704.) entitled, A Treatiſe of Chirurgery, with an Herbal, &c. in Old Engliſh, 4to. 1438. And another,

(N° 7709.) called, An Herbary, &c. written alphabetically, according to the Latin names, in 1443. And

(N° 7537.) entitled, A Book of Plants and Animals, delineated in their natural colours on velom, Old Engliſh, A. D. 1504.

Mr. Ames, in his Typographical Antiquities, p. 470, informs us, that, in the year 1516, a folio, entitled, " The Greate Herball," was printed in

(*d*) Bibliotheca, p. 48.
(*e*) Cat. MSS. Angl. p. 341.

Southwark by Peter Treveris; and this, Sir, I believe, is the oldeſt Engliſh herbal now extant in print.

To come to later times. The ingenious Mr. Gough (in his Britiſh Topography, p. 61.) informs us, " That, before the year 1597, John Gerrard, " citizen and ſurgeon of London, ſeems to be the " firſt who cultivated a large phyſic garden, which " he had near his houſe in Holborn, where he " raiſed 1100 different plants and trees." (He might have added, that Gerrard had another phyſic garden in Old-ſtreet, containing a great variety of plants; a printed catalogue of which is to be found in the libraries of the curious). But Gerrard had a famous cotemporary, who greatly advanced that valuable ſcience, and of whom but little hath hitherto been ſaid by the modern biographers.

John Tradeſcant is the perſon meant. And I hope, Sir, that an attempt to revive the memory of this once eminent botaniſt and virtuoſo will not be diſpleaſing.

John Tradeſcant was, according to Anthony Wood, a Fleming, or a Dutchman. We are informed by Parkinſon, that he had travelled into moſt parts of Europe, and into Barbary; and, from ſome emblems remaining upon his monument in Lambeth church-yard, it plainly appears that he had viſited Greece, Egypt, and other Eaſtern countries.

In his travels, he is ſuppoſed to have collected not only plants and ſeeds, but moſt of thoſe curioſities of every ſort, which, after his death, were ſold by his ſon to the famous Elias Aſhmole, and depoſited in his Muſeum at Oxford.

When

When he firſt ſettled in this kingdom, cannot, at this diſtance of time, be aſcertained; perhaps it was towards the latter end of the reign of Queen Elizabeth, or the beginning of that of King James the Firſt. His print, engraven by Hollar before the year 1656, which repreſents him as a perſon very far advanced in years, ſeems to countenance this opinion.

He lived in a great houſe at South Lambeth, where there is reaſon to think his muſeum was frequently viſited by perſons of rank, who became benefactors thereto: among theſe were King Charles the Firſt (to whom he was gardener), Henrietta Maria his Queen, Archbiſhop Laud, George Duke of Buckingham, Robert and William Cecil, Earls of Saliſbury, and many other perſons of diſtinction (*f*).

John Tradeſcant may therefore be juſtly conſidered as the earlieſt collector (in this kingdom) of every thing that was curious in Natural Hiſtory, viz. minerals, birds, fiſhes, inſects, &c. &c. He had alſo a good collection of coins and medals of all ſorts, beſides a great variety of uncommon rarities. (*g*) A catalogue of theſe, publiſhed by his ſon, contains an enumeration of the many plants, ſhrubs, trees, &c. growing in his garden, which was pretty extenſive. Some of theſe plants are (as I am informed), if not totally extinct, at leaſt become very uncommon, even at this time. A liſt of ſome remarkable ones

(*f*) See a liſt of them at the end of *Muſeum Tradeſcantianum*, 12mo, London, 1656—where are Hollar's two prints of John Tradeſcant, the father and ſon.

(*g*) Ibid.

introduced by him, is inſerted below (*b*). And this able man, by his great induſtry, made it manifeſt (in the very infancy of Botany), that there is ſcarce

(*b*) From *Parkinſon's Garden of pleaſant Flowers*, printed in 1656.

1. " Pſeudonarciſſus aureus maximus flore pleno, ſive roſeus
" Tradeſcanti.
" The greateſt double yellow baſtard daffodil, or John
" Tradeſcant's great roſe daffodil.
" This daffodil was primarily introduced by John Tradeſ-
" cant, and for its extreme beauty, may well be entitled
" the glory of daffodils." . . . . . Page 102.
2. " Moly Homericum, vel potius Theophraſti.
" The greateſt moly of Homer. . . . . 141.
3. " Moly Indicum, ſive Caucaſon.
" Indian moly. . . . . *ibid.*
" Both the above molys are natives of Spain, Italy, and
" Greece, and were procured from thence by John Tradeſ-
" cant, and flouriſhed with him, in his garden at Can-
" terbury," (ſhould be South Lambeth).
4. " Ephemerum Virginianum Tradeſcanti.
" John Tradeſcant's ſpider-wort of Virginia.
" This ſpider-wort is of late knowledge, and for it the
" Chriſtian world is indebted unto that painful induſtri-
" ous ſearcher and lover of all nature's varieties John
" Tradeſcant." . . . . . 152.
5. " Gladiolus Byzantinus.
" Corn-flag of Conſtantinople.
" With this ſpecies John Tradeſcant obſerved many acres of
" ground in Barbary overſpread. . . . . 190.
6. " Elleborus albus vulgaris.
" White hellebore.
" This groweth in many places in Germany, and alſo in
" ſome parts of Ruſſia, and in ſuch plenty, that John
" Tradeſcant obſerved quantity ſufficient to load a good
" ſhip with the roots. . . . . 346.
7. " Nardis montana tuberoſa.
" Knobbed mountain valerian.
" Diſcovered in a botanic excurſion by J. Tradeſcant. 388.

any

any plant extant in the known world, that will not, with proper care, thrive in this kingdom.

When his houſe at South Lambeth (then called Tradeſcant's Ark) came into Aſhmole's poſſeſſion, he added a noble room to it, and adorned the chimney with his arms, impaling thoſe of Sir William Dugdale (whoſe daughter was his third wife), where they remain to this day.

This houſe belongs at preſent to John Small, Eſq; who, about twelve-years ago, purchaſed it of ſome of Aſhmole's deſcendants; and my houſe, once a part of Tradeſcant's, is adjoining thereto.

It were much to be wiſhed, that the lovers of Botany had viſited this once famous garden, before, or at leaſt in, the beginning of the preſent century. But this ſeems to have been totally neglected till the year 1749, when yourſelf, and the late Dr. Mitchel, favoured the Royal Society (*i*) with the only account now extant, of the remains of Tradeſcant's garden. In it, Sir, you ſeem to confine the extent thereof to

8. "John Tradeſcant introduced a new ſtrawberry, with "very large leaves, from Bruſſels; but, in the courſe "of ſeven years, could never ſee one berry completely "ripe. . . . . Page 528.
9. "John Tradeſcant procured a new and great variety of "plums from Turky, and other parts of the world. 575.
10. "The Argier, or Algier apricot. This, with many other "ſorts, John Tradeſcant brought with him, returning "from the Argier voyage, whither he went with the "fleet that was ſent againſt pirates, A° 1620." 579.

Thus far Parkinſon; but whether or no theſe plants bear his name at this period, I can no more pretend to aſſert, than that all the ſpecies therein mentioned are even now exiſting in our gardens.

(*i*) Philoſophical Tranſactions, Vol. XLVI. p. 160.

that

that now belonging to Mr. Small's houſe. I believe it was otherwiſe; and, on the account of the great number of plants, trees, &c. am inclined to think that Tradeſcant's garden extended much farther. Bounded on the Weſt by the road, on the Eaſt by a deep ditch (ſtill extant) it certainly extended a good way towards the North, and took in not only my orchard and garden, but alſo thoſe of two or three of my next neighbours; and ſome ancient mulberry trees, planted in a line towards the North, ſeem to confirm this conjecture.

When the death of John Tradeſcant happened, I have not been able to diſcover, no mention being made thereof in the Regiſter Book of Lambeth Church.

A ſingular monument (of which I herewith ſend you a drawing, Tab. IV. and V.) was erected, in the South-Eaſt part of Lambeth church-yard, in 1662, by Heſter, the relict of John Tradeſcant the ſon, for himſelf, and the reſt of this family, which is long ſince extinct (*k*).

This once beautiful monument hath ſuffered ſo much by the weather, that no juſt idea can now, on inſpection, be formed of the North and South ſides. But this defect is happily ſupplied from two fine drawings, preſerved in Mr. Pepys's Library at Cambridge. We ſee

On the Eaſt ſide Tradeſcant's arms.
On the Weſt A hydra, and under it a ſkull.

(*k*) John the grandſon, buried 15th September 1652.
John the ſon, buried 25th April 1662.
Heſter, widow of John Tradeſcant, buried 6th April 1678.
From the Regiſter of Lambeth Church.

On

On the South { Broken columns, Corinthian capitals, &c. ſuppoſed to be ruins in Greece, or ſome other eaſtern countries.

On the North { A crocodile, ſhells, &c. and a view of ſome Egyptian buildings.

Various figures of trees, &c. in relievo adorn the four corners of this monument.

The following remarkable epitaph (preſerved at Oxford, and printed in Mr. Aubrey's Antiquities of Surrey, p. 11.), was intended for, but never placed upon, this monument.

Know, ſtranger, e'er thou paſs, beneath this ſtone
Lie John Tradeſcant, grandſire, father, ſon.
The laſt dy'd in his ſpring; the other two
Liv'd till they had travelled art and nature thro'.
As by their choice collections may appear,
Of what is rare in land, in ſeas, in air:
Whilſt they (as Homer's Iliad in a nut)
A world of wonders in one cloſet ſhut.
Theſe famous antiquarians that had been
Both gardiners to the Roſe and Lilly Queen,
Tranſplanted now themſelves, ſleep here; and when
Angels ſhall with their trumpets awaken men,
And fire ſhall purge the world, theſe hence ſhall riſe
And change their garden for a paradiſe.

Before I conclude, I muſt beg leave to add a liſt of the portraits of the Tradeſcant family, now in the Aſhmolean Muſeum. I cannot, however, conceive why both father and ſon are therein called Sir John, as it does not appear either of them were ever knighted. But ſo it is in the Oxford liſt communicated

cated to me, ſome time ſince, by the late worthy and learned Mr. William Huddesford, keeper of the Aſhmolean Muſeum.

1. Sir John Tradeſcant, ſenior. A three quarter piece, ornamented with fruit, flowers, and garden roots.
2. Ditto. After his deceaſe.
3. A ſmall three quarter piece. Water colours.
4. A large piece, of his wife, ſon, and daughter. Quarter length.
5. Sir John Tradeſcant, junior, in his garden. Half length, a ſpade in his hand.
6. Ditto, with his wife, in one piece. Half length.
7. Ditto, with his friend Zythepſa of Lambeth, a collection of ſhells, &c. upon a table before them. A large quarter piece, inſcribed Sir John Tradeſcant's ſecond wife, and ſon.

Theſe pictures have no date, nor painter's name, as I can yet find. They are eſteemed to be good portraits. Who the perſon was, called in the picture Zythepſa, I never could learn. He is painted as if entering the room, and Sir John is ſhaking him by the hand.

I have the honour to remain, with great eſteem,

SIR,

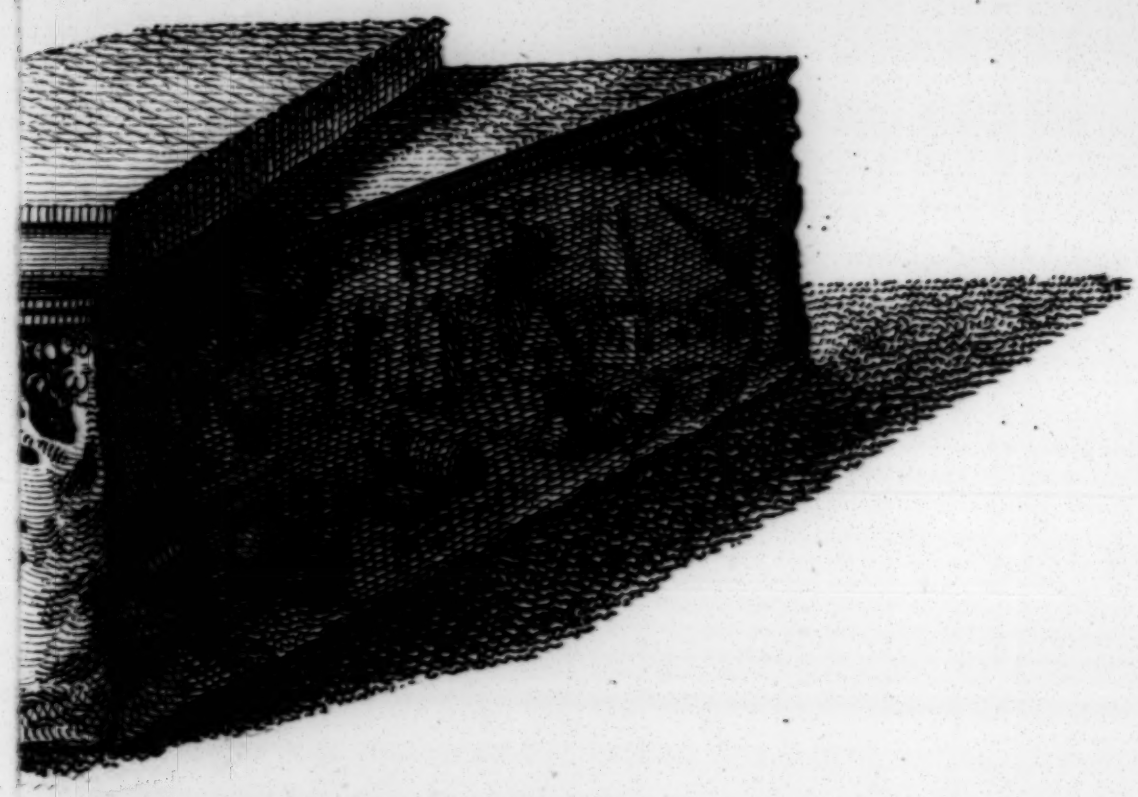
Your moſt faithful,

humble ſervant,

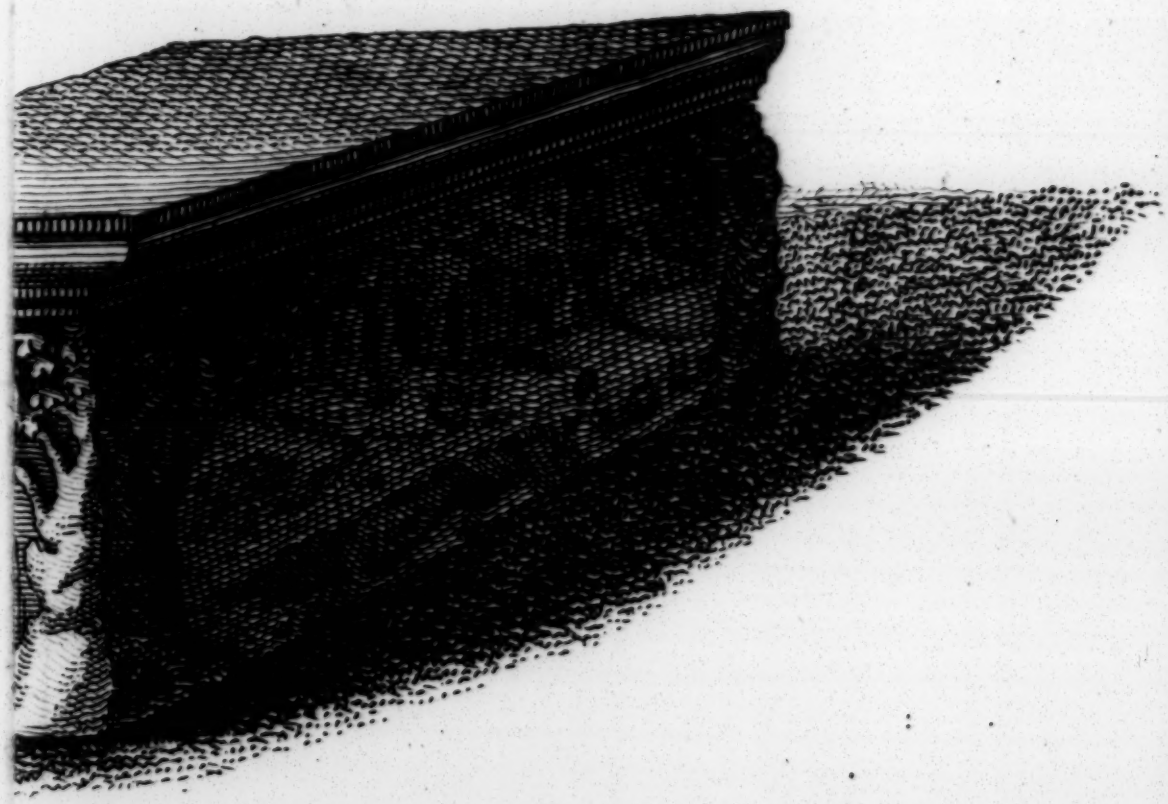
Doctors-Commons,
November 2, 1772.

And. Coltee Ducarel.

m the West.



om the East.



ent of JOHN TRADESCANT in the Church
Mary Lambeth 1773.

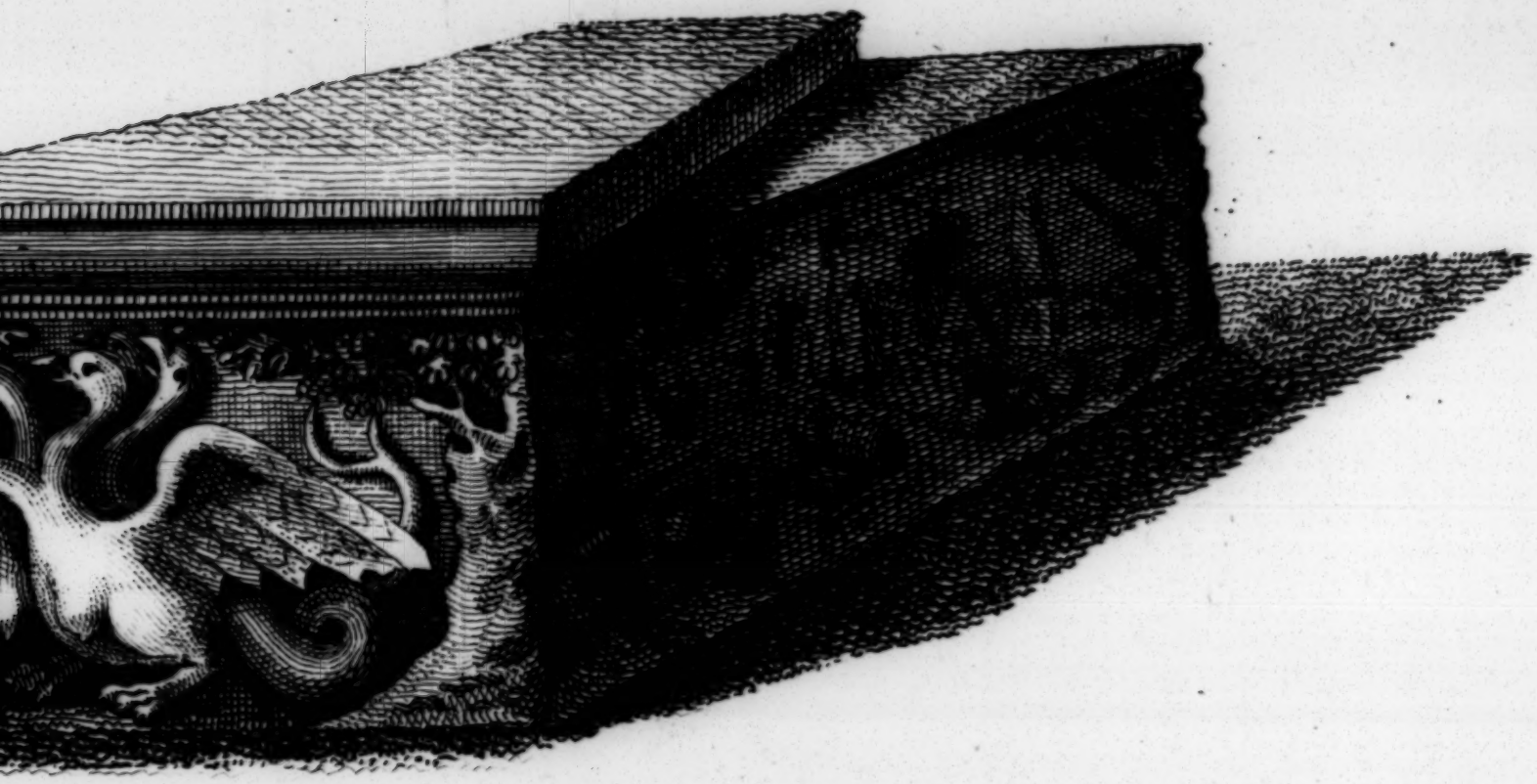
Basire sc.



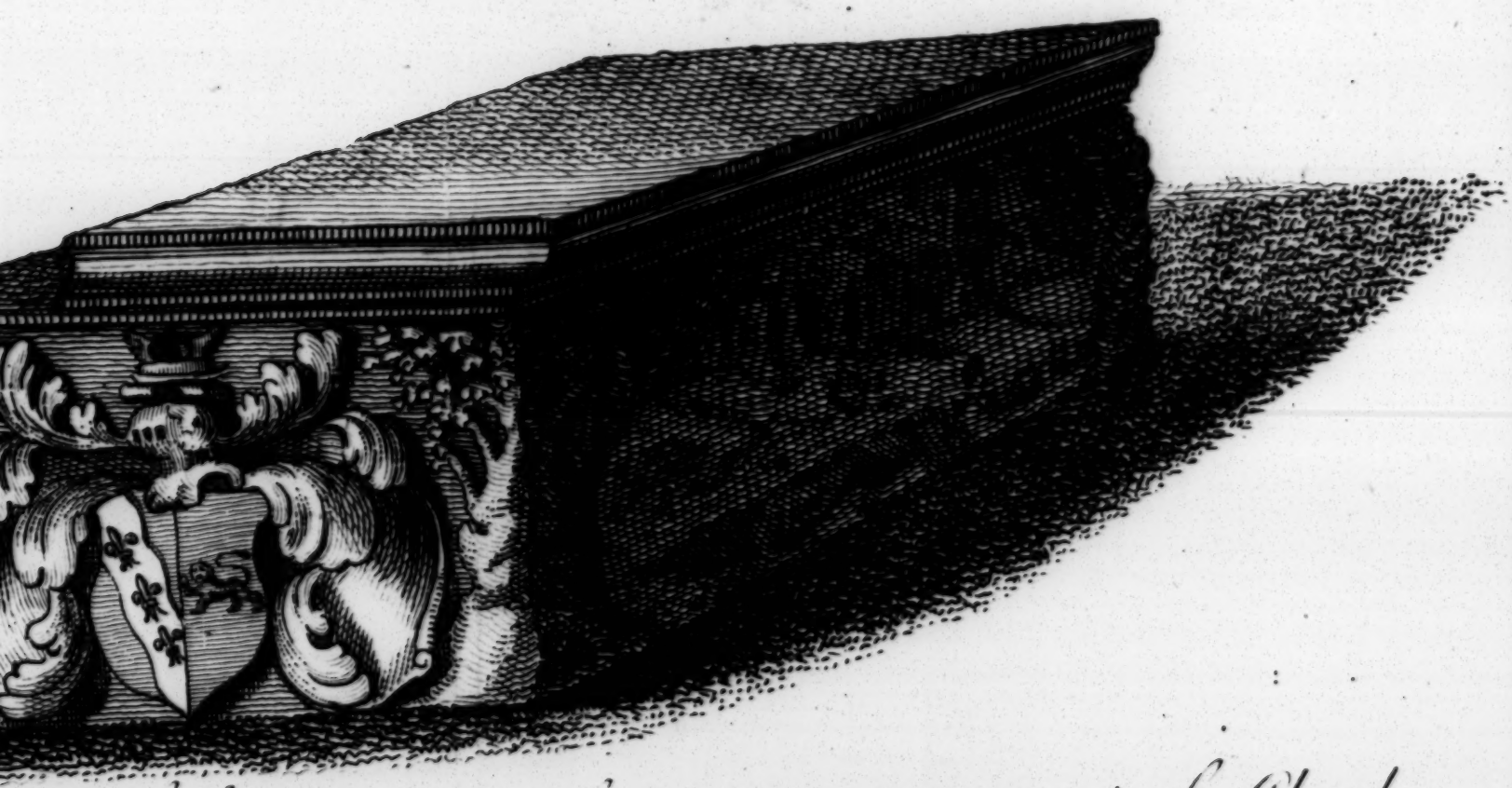
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From the West.



From the East.



...iews of the Monument of JOHN TRADESCANT in the Church Yard of St. Mary Lambeth 1773.

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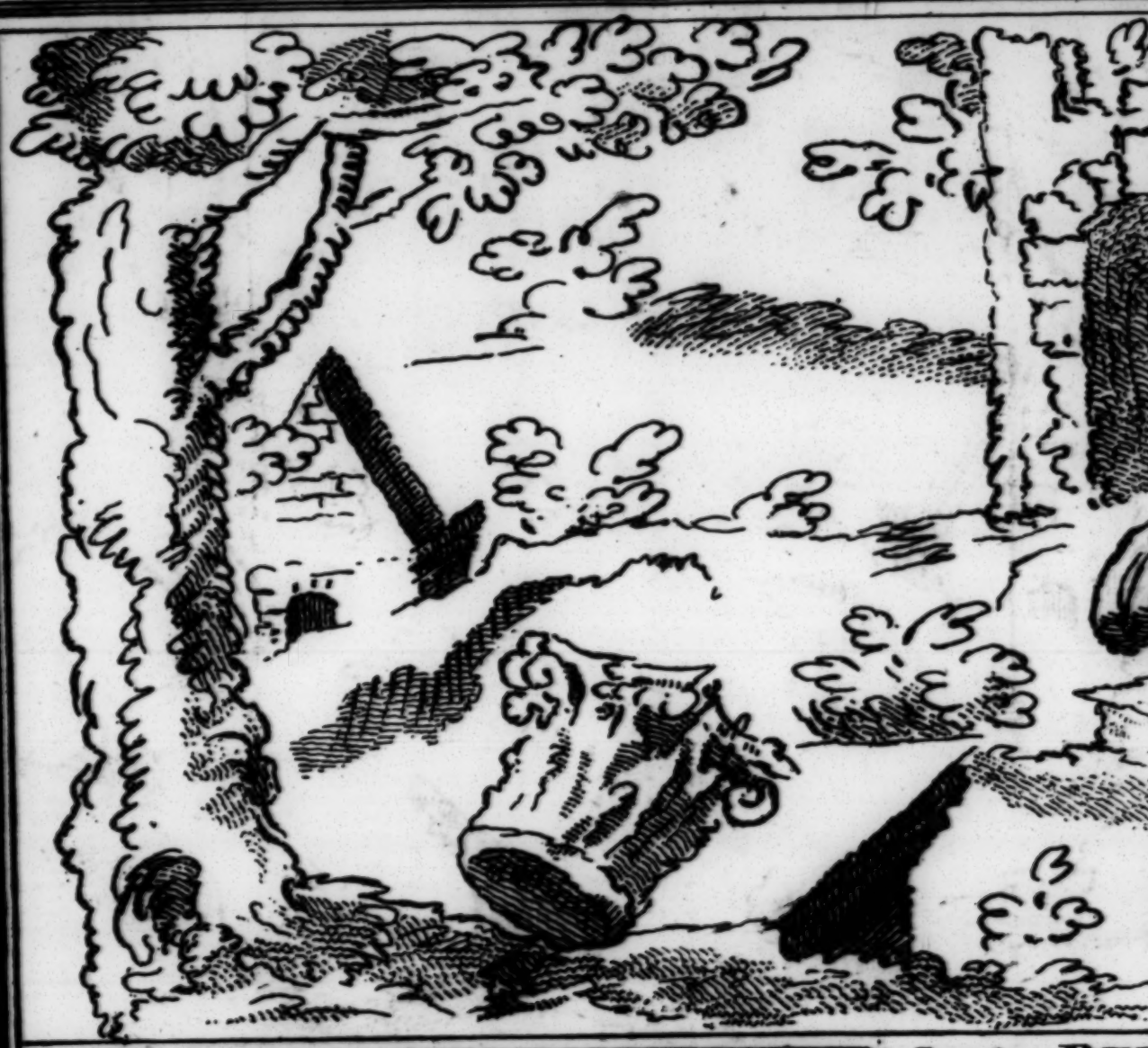


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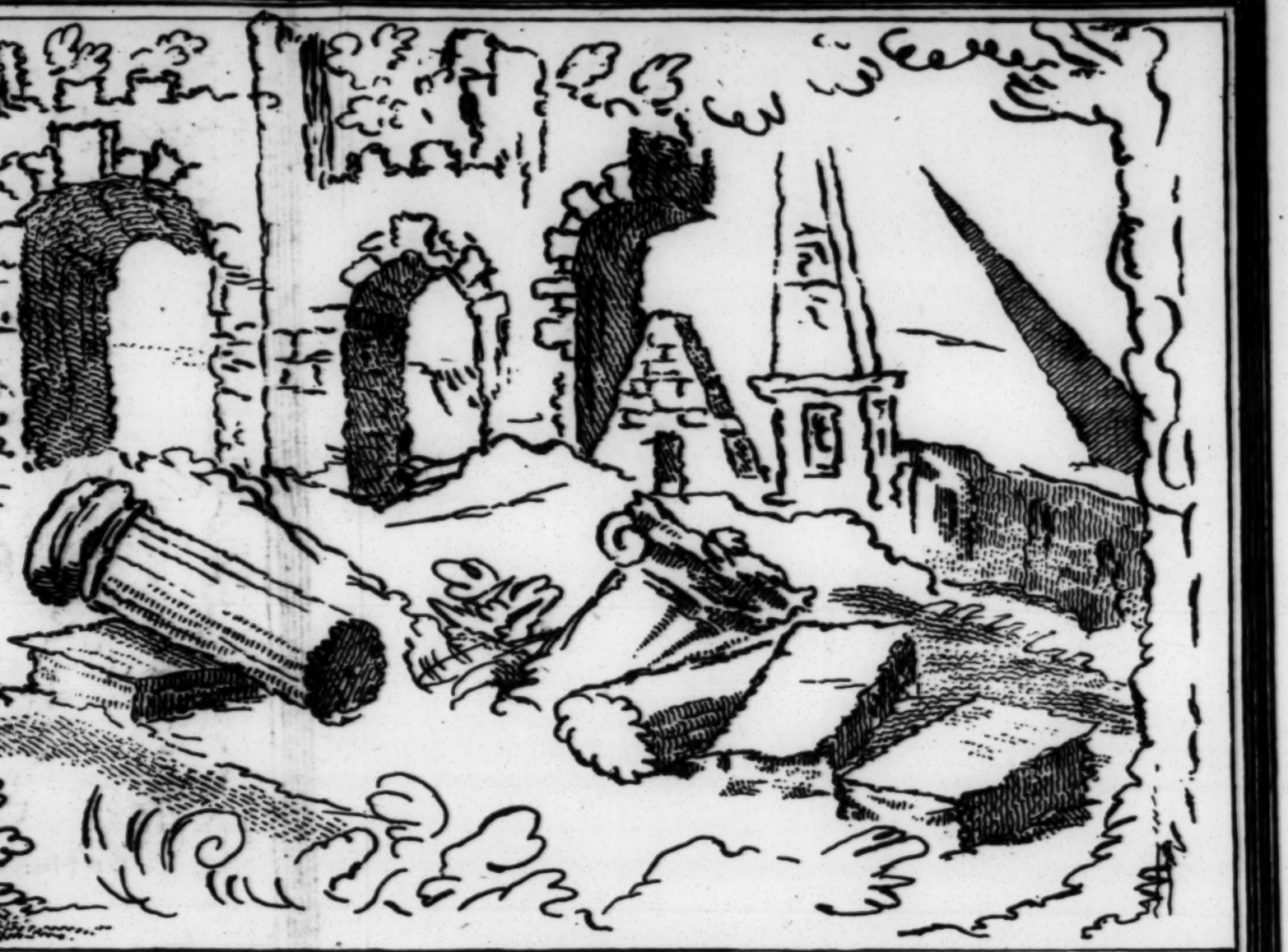
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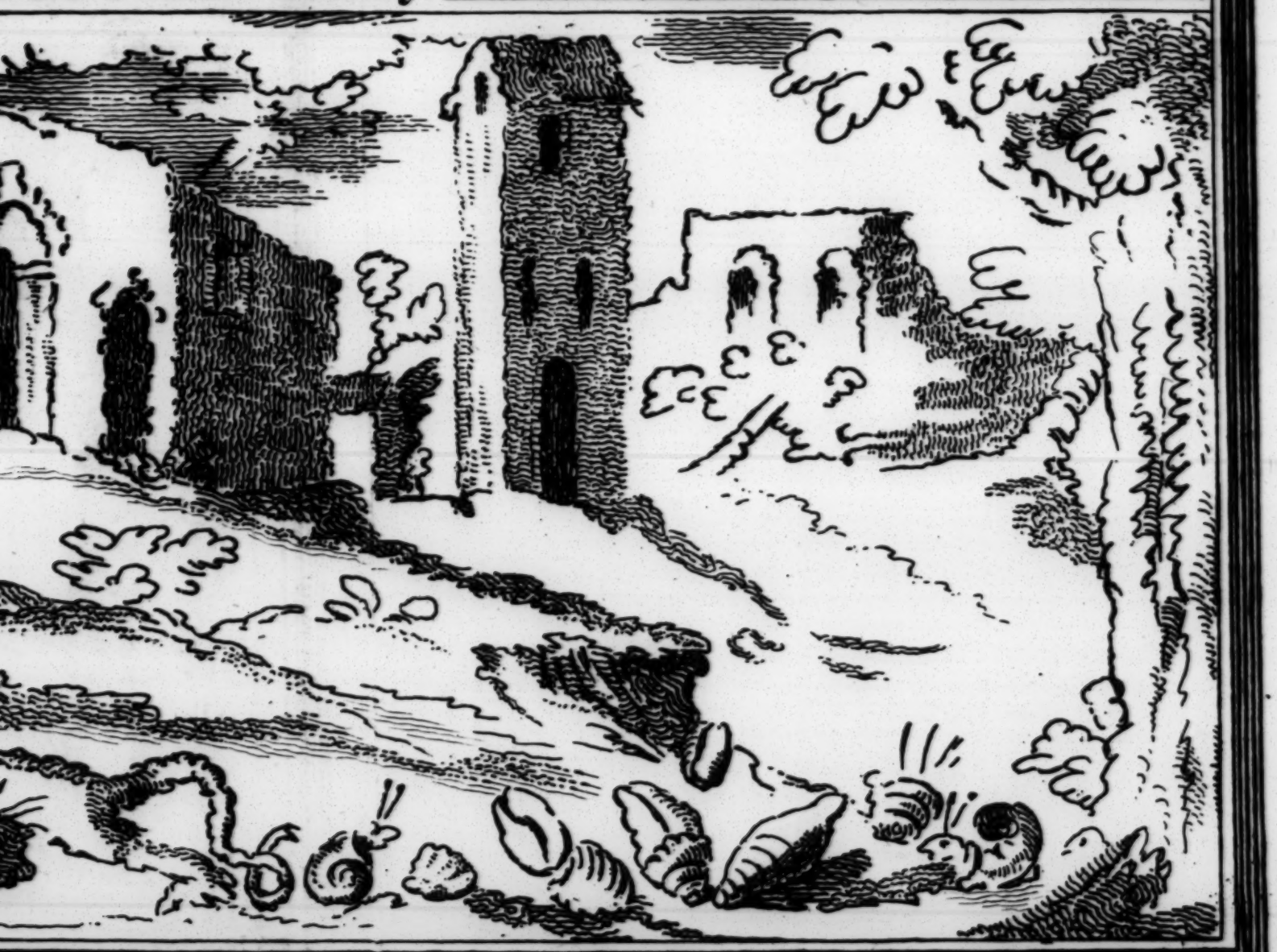
From

m the SOUTH.

Philos. Trans. Vol. LXIII. Tab. V. p. 88.



PEPYSIAN Library.



om the NORTH

Baurer sc.

XIII. *De intenſo Frigore Menſibus* Januario 1767 *ac* 1768, & Novembri 1770, *obſervato* Franequeræ, *Auctore* J. H. Van Swinden, *Philoſ. Profeſſ. in Academia* Franeker. *Societ.* Harlemo-Hollandicæ *Socio.*

IN Tranſactionum Philoſophicarum, tomis LVIII. & LIX. vidi nuperrime, illuſtriſſimos horum Actorum Collectores obſervata quædam inſeruiſſe circa acerbum frigus anni 1768: cum vero exinde liqueat hujuſmodi annotationes uſu non carere, in medium proferam quæ, vel propriis vel alienis obſervationibus, in patria inſtitutis, ad notitiam meam pervenerunt.

Anno 1767, peritiſſimus teleſcopiorum catoptricorum artifex Vander Bild, thermometrum Fahrenheitianum obſervavit,

Jan. 6°.	h. 8	A. M. ad gr. . . .		16
	h. 10	P. M.		2
7°.	h. 7½	A. M.	—	2
	12	. . . . .	+	12
	7	P. M.		5
	9	. . . . .		7
8°.	7½	A. M.		12

Minima altitudo fuit ad minimum — 2, nam mercurius totus intra bulbum erat reconditus, adeoque ad minimum ad 2 gr. infra o, cum bulbus a o hac quantitate diſtet. Mercurius ab alio hic viſus fuit ad — 5, ut mihi memoratus Vander Bild narravit. Si vero comparationis tabellam formemus, habebimus,

	Franeq.	Amſt.	Leidæ.	Zwaneburgi.	Wel.
Die 7° Jan.	— 2	+ 4	5	6	— 3

Hæ autem obſervationes omnes, mane hora 7 aut 8 inſtitutæ fuerunt, excepta ea quam in pago Wel, prope Silvam Ducum inſtituit Rev. Palier: ibi therm. fuit,

h. 7.	A. M.	ad	— 2½	h. 2.	ad	7½	h. 9.	P. M.	— 3
8.	. . .		— 2	6.		o	10.	. . .	o
9.	. . .		o	7.		— 1			
10.	. . .		1	8.		2			

(Videat. Harlem. Societ. Act. Belg. T. IX. p. 649. Partis III.). Notatum autem eſt maximum frigus A° 1740, in Hollandia fuiſſe — 2, ac — 4.

A° 1768. Obſervationes a meipſo inſtitutæ ſunt. Die Jan. 3, altitudo maxima, h. 12 ad gr. 13, minima gr. 7; h. P. M. 5. vento N. O. die 4, maxima, 20°, minima 12, vento O. ac Z. O. die 5, maxima 23°, minima 18, h. 11. P. M. vento Z. Z. O; ninxit hoc die. Die vero 6, hora 8¼ A. M. ther. 3½, Vento Z. O. cœlo ſereno: hora 9, ad 6°, h. 11½ ad 6°, h. 11, ad 12; h. 4. P. M. ad 10, hora 10 ad 11, cum valido vento orientali. Si iterum comparationem inſtituamus, habebimus,

Franeq.

Franeq	Amſter.	Zwanenb.	Leidæ.	Wel.
3½	7	6 Die 3.	4	—3½ Die 3.
Die 6.	Die 4.	7 Die 4.	Die 4.	—1 Die 4.
		8 6.	8 Die 6.	+7 Die 6.

Aliis in locis, uti Lipſiæ, Coloniæ, Regiomonti, &c. mercurium multum infra zero deſcendiſſe novellæ publicæ retulerunt. Sane ſi ad frigoris intenſitatem & durationem adtendamus, hanc hiemem inter acerbiſſimas referemus: ſed hæc hactenus, tantum enim maximos frigoris gradus adnotare volui.

Verum quæ menſe Novembri 1770, hic obſervavi rariora ſunt.

Die Nov. 17, gelu cum nive incepit, vento O. Die 18, h. 8, therm. 27½. Die 19, h. 8. A. M. 24°, hora 2, P. M. 28; h. 5, 19°: h. 10, 16°. vento O, ac O. Z. O. At die 20, h. 7½. therm. 9°. vento Z. Z. O. dein vero celeriter adſcendit: nam hora 2, erat jam ad 25°. vento O. Z. O. cœlo ſereno: h. 6, ad 32°, vento Z. Z. W. cum nive, hora 10, ad 32°. die 21, h. 10, ad 36°. Neſcio an noſtris in regionibus, tanti frigoris hoc menſeobſervati dentur exempla. An imminens nix ejus cauſa exſtitit? Si vero comparationem cum aliis locis inſtituamus, habebimus,

Franeq.	Amſt.	Zwanenb.	Leidæ.	Hag.	Berol.
9	22	25 Die 17, 23.	17 h. 11, P. M. 36.	21	17¾

Maxima mutatio hic fuit 27°. Magnam autem inter loca, haud multum ab invicem diſſita, differentiam dari liquet, & ſæpe fuit obſervatum: omnis

 tamen

tamen a ſola thermometrorum ſitu vario repeti nequit.

Ad hanc frigoris intenſitatem accedit, celeremque hanc temperiei mutationem multum ſuperat, quod in vico Tooting, die 22 Nov. 1748, obſervavit Rev. Miles (Philoſ. Tranſact. N° 491. T. XLVI. p. 3.), ſc. Die 21 Nov. fuit therm. ad 7° gr. infra o, ſeu congelationis punctum. Die 22, h. 4⅓, A. M. 14½ infra o. h. 7, gr. 5 infra o, vento W. ac Z. W. Mane, hora 10, jam pluebat thermom. indicante 5 ſupra o, hora 8¼, P. M. therm. erat ad 12 ſupra o. Unde mutatio fuit gr. 26½. Ait vero Cel. Maty (Jour. Britan. T. V. p. 124.), hoc therm. cum Fahrenheitiano convenire, niſi quod punctum o in puncto congelationis fuerit notatum. Unde in ſcala Fahrenheitiana erunt frigoris gradus hi.

22 Nov. 1748, h. 4⅓ gr. 17½. h. 7. gr. 27. h. 10, gr. 37. cum pluvia, h. 8½, P. M. gr. 42. Frigus ergo minus eo quod hic obſervavi.

Franeq. 13 Nov. 1772. J. H. Van Swinden.

P. S. Anno 1771, die Jan. 13, vidi therm. ad 8 gr. die Feb. 11, ad gr. 13. die Jan. 19, 1772, ad 11°. Verum de his alia opportunitate latius.

XIV. *An*

XIV. *An Inquiry into the Quantity and Direction of the proper Motion of* Arcturus; *with ſome Remarks on the Diminution of the Obliquity of the Ecliptic: By* Thomas Hornſby, *M. A.* Savilian *Profeſſor of Aſtronomy in the Univerſity of* Oxford, *and F. R. S.*

Read Dec. 24, 1772.

AS an accurate knowledge of the *poſition of the fixed ſtars* is of the greateſt importance, being the baſis and foundation of aſtronomy, it is no wonder that the aſtronomers of different nations have given great attention to this matter. By comparing *antient* with the beſt *modern obſervations*, it appears that ſome of the fixed ſtars have a *proper motion*, independent of any motion hitherto known in our own ſyſtem; or that, in other words, the *angular diſtances of the fixed ſtars have not always continued the ſame*, and in ſome of them the alteration is ſo very conſiderable as to be eaſily perceived in the courſe of *a few years*, with inſtruments accurately made, and nicely adjuſted. Of all the ſtars viſible in our hemiſphere, the *variation* in the place of *Arcturus is the moſt remarkable*, and ſuch as cannot poſſibly be attributed to the uncertainty of obſervation. It has accordingly been taken no-

tice of by many aſtronomers: in particular, Dr. *Halley* mentions it in N° 355 of the Philoſophical Tranſactions: Mr. *Caſſini*, in the Memoirs of the Academy of Sciences for 1738, p. 231, has ſhewn, that there is a variation of *five minutes* in the latitude of that ſtar between his own time and that of Tycho, in an interval of a *century and a half:* and M. le *Monnier*, in the Memoirs of the Academy of Sciences for 1767, p. 417, proves, that the *latitude* of Arcturus varies at the rate of *two ſeconds* every *year*; and that the *longitude* decreaſes at the rate of 60″ in a hundred years *. But as an *inquiry* as well into the true *quantity* as into the *direction* of this motion has not hitherto been made public, I propoſe to give ſome account of my own obſervations made expreſsly with this view in the years 1767 and 1768, with a tranſit inſtrument of 44 inches, and a moveable mural quadrant of 33 inches, both conſtructed by Mr. Bird, and of the concluſions reſulting from a compariſon between them and ſome obſervations made by Mr. Flamſteed in 1690.

It may perhaps be objected, that the differences of right aſcenſion, as determined by Mr. Flamſteed's mural inſtrument, are not to be depended upon from the very nature of his inſtrument. Mr. Flamſteed was himſelf too good an obſerver not to be aware of this; and accordingly in the Prolegomena to the third volume of the *Hiſtoria Cæleſtis*, p. 132, he informs us in what manner he determined the error of

* See alſo the Memoirs of the Academy of Sciences for 1769, p 21. See alſo Aſtronomiæ Fundamenta, by the Abbé de la Caille; who, in reducing his obſervations of Arcturus, ſuppoſes the annual motion of declination in that ſtar = 19″,0. p. 169, and 187.

the plane at different diſtances from the zenith. By diſtributing theſe errors in the beſt manner I could, I am of opinion, that the error of the plane of his inſtrument may be ſuppoſed to decreaſe uniformly at the rate of half a ſecond in time for every degree of zenith diſtance from 28° to 60°, the error being 39″ at the former, and 23′ at the latter, by which quantity ſtars paſſed the horary wire, in his inſtrument, before they came to the true meridian. It ſhould ſeem alſo that the error continued nearly the ſame from 60 to 75 degrees, being at the latter only 22″: but that it decreaſed irregularly from 75° to 85°, viz. 1″ in time for each degree from 75° to 80°, and 0″,4 for each degree from 80° to 85 degrees. The mural arc was fixed upon a ſtone pier, the ſouthern part of which was found to ſettle yearly, from whence the error of the line of collimation to the ſouth neceſſarily became every ſucceſſive year greater and greater. As Mr. Flamſteed ſeems not to have had any method of adjuſting his inſtrument by a plumb-line, theſe errors muſt have been irregular at different ſeaſons of the ſame year, and were perhaps never truly determined. But as the obſervations here referred to were made on the ſame day, and within the compaſs of an hour, they are probably not affected with this latter error. We are at preſent concerned with the difference of two zenith diſtances, and not with the abſolute quantity of thoſe zenith diſtances. The concluſions may indeed be affected with an error in the diviſions; and from the examination which I have been able to make, I am of opinion that the arc of Mr. Flamſteed's inſtrument was not of the proper quantity; and that, though the obſervations generally

generally erred in defect, they in ſome parts erred in exceſs.

On the 14th of February, 1690, Mr. Flamſteed obſerved, that a ſmall ſtar, of the ſeventh or eighth magnitude, whoſe place is not determined in the Britiſh catalogue, and which ſtar was named by him *Infra Arcturum*, preceded Arcturus three ſeconds in time, or 3″,3, when an allowance is made for the error of the plane of the inſtrument = 0′ 42″, 6, and was 26′ 30″ to the ſouth of Arcturus *. By a mean of eight obſervations made at Oxford, on and near June the 10th, 1767, with the tranſit inſtrument, and with a refracting teleſcope of eight feet, furniſhed with a micrometer; the difference of right aſcenſion was 1′, 8″,75 of a degree, the ſtar following Arcturus; and by a mean of three obſervations, the extremes differing only 3″, the ſmall ſtar was 23′ 55″,0 to the ſouth of Arcturus.

The right aſcenſion of Arcturus and the ſmall ſtar being nearly the ſame, the change in declination ought to be ſo likewiſe. But, from the obſerved difference in declination, the right aſcenſion of the two ſtars muſt vary unequally, though with a very ſmall difference. Accordingly it appears from computation (in which the annual preceſſion is ſuppoſed = 50″,35, the obliquity of the ecliptic at the middle of the interval of the time = 23° 28′ 30″, and the right aſcenſions and declinations of the two ſtars taken at a mean between the times of obſervation) that the variation of Arcturus in right aſcenſion was 3270″,6, and of the ſmall ſtar 3277″,6 in

* This is the only obſervation of that ſtar made by Mr. Flamſteed.

77,287 years. Therefore the right aſcenſion of Arcturus alters leſs than that of the ſtar; and conſequently Arcturus ſhould in 1767 have followed the ſtar by 42″,6. But the ſtar was obſerved to follow Arcturus by 1′ 8″,75. The right aſcenſion therefore of Arcturus has increaſed leſs than that of the ſtar, or Arcturus has moved weſtward 1′ 51″,35 in 77,287 years; and has gone ſouthward 2′ 35″ in the ſame time, ſuppoſing the ſmall ſtar not to have moved, which is highly probable.

On the ſame day the difference of right aſcenſion in time between the ſtar η Bootis and Arcturus was 21′ 32″ of mean ſolar time, $= 5^{\circ}$ 24′ 02″,2, when a proper allowance is made for the going of the clock, and for the error of the plane of the inſtrument; and the difference of declination was 50′ 45″,6, when an allowance is made for refraction. On the 24th, 26th, and 29th of May, and the 9th of June, of the year 1768, I determined the difference in right aſcenſion to be 21′ 27″ of ſidereal time by the two former obſervations, and 21′ 26¾″ by the two latter, the difference in declination being 49′ 48″,7, by a mean of the obſervations in May, the extremes differing only four ſeconds. It appears from computation, that between the times of obſervation the variation of η Bootis in right aſcenſion was 3371″,7, and 1417″,3, in declination; of Arcturus 3311″,7 in right aſcenſion, and 1347″,9 in declination: The difference of variation in right aſcenſion is 1′ 0″, and of declination 1′ 9″,4; by the former the difference in right aſcenſion was diminiſhed, and in declination increaſed by the latter, agreeably to the places of the two ſtars. The difference in right aſcenſion

therefore in 1768, if neither of the ſtars had moved, ſhould have been = 5° 23′ 02″,2, and 51′ 55″ in declination; but they were obſerved to be 5° 21′ 43″,4, and 49′ 48″,7. Arcturus therefore by this obſervation has in 78,257 years gone 1′ 18″,8 to the weſt, and 2′ 06″,3 to the ſouth, ſuppoſing η Bootis not to have any proper motion.

On the 5th of April, 1691, the difference in right aſcenſion between η Bootis and Arcturus was 21′ 33″ of mean ſolar time, = 5° 24′ 14″,0; and the difference of declination 50′ 45″,6, as in the preceding example. The difference of variation in right aſcenſion is 59″,1, and in declination 1′ 8″,4. The difference of right aſcenſion therefore at the latter end of May, 1768, ſhould have been 5° 23′ 14″,9, and 51′ 54″,0 in declination; but, according to obſervation, they were 5° 21′ 43″,4, and 49′ 48″,7. Arcturus therefore, according to this obſervation, has moved 1′ 31″,5 to the weſt, and 2′ 05″,3 to the ſouth in 77,120 years.

On the 4th of May, 1691, the difference of right aſcenſion between η Bootis and Arcturus was 21′ 33″ of mean ſolar time, = 5° 24′ 14″,3 when allowance is made for the going of the clock and the error of the plane of the inſtrument, and the difference of declination on the 3d of May = 50′ 50″,6. According to computation, thoſe differences ſhould have been 5° 23′ 15″,2 and 51′ 59″,0 reſpectively; but they were obſerved to be 5° 21′ 43″,4, and 49′ 48″,7. Arcturus therefore in 77,071 years has moved 1′ 31″,8 weſtward, and 2′ 10″,3 ſouthward. N. B. The zenith-diſtance of Arcturus, as

as determined by Mr. Flamſteed, on the 4th of May, is manifeſtly erroneous.

On the 27th of May, 1692, η Bootis preceded Arcturus in right aſcenſion by 21′ 32″,5 of mean ſolar time, = 5° 24′ 10″,1, the difference of declination being 50′ 50″,6. In 75,978 years the difference of right aſcenſion ſhould have been 5° 23′ 11″,8 and 51′ 58″,0 in declination; but thoſe differences were obſerved to be 5° 21′ 43″,4 and 49′48″,7. Arcturus therefore has moved 1′28″,4 weſtward, and 2′ 09″,3 ſouthward.

On the 27th of May, 1692, Arcturus preceded π Bootis in right aſcenſion by 24′ 35″,5 of mean ſolar time, = 6° 9′ 32″,2, when an allowance is made for the going of the clock and the error of the plane of the inſtrument, the difference of declination being 3° 2′ 28″,9. On the 24th and 26th of May, and 5th of June, 1768, the difference of right aſcenſion between the ſame ſtars obſerved at Oxford was 24′ 44″,58 of ſidereal time, = 6° 11′ 9″,1, the difference of declination being 2° 58′ 24′,2. In 1768, the difference of right aſcenſion ſhould have been 2″,7 greater, = 6° 9′ 34″,9; and the difference of declination 1′ 31″,7 leſs, = 3° 0′ 57″,2. But they were obſerved to be 6° 11′ 9″,1, and 2° 58′ 24″,2. Arcturus therefore in 75,978 years has, by a compariſon with this ſtar, moved 1′ 34″,2 weſtward, and 2′ 33″,0 ſouthward.

Again, the difference of declination between Arcturus and π Bootis was obſerved to be 3° 2′ 33″,9 on the 14th of February, 1690, when the difference of right aſcenſion between theſe two ſtars was not obſerved by Mr. Flamſteed. It appears by compu-

tation, that the difference of variation in declination between the times of obſervation was 1′ 34″,5, by which quantity the difference of declination was diminiſhed, and ſhould therefore in 1768 have been 3° 0′ 59″,4. But it was 2° 58′ 24″,2 by actual obſervation. Arcturus therefore by this obſervation has moved ſouthward 2′ 35″,2 in 78,255 years.

By the foregoing compariſons Arcturus appears to have moved as in the following table.

	years.	Weſtwd. ′ ″	Southwd. ′ ″
By the ſmall ſtar Feb. 14, 1690, in	77,237	1 51,35	2 35,0
η Bootis Feb. 14, 1690, in	78,257	1 18,8	2 6,3
η Bootis Apr. 5, 1691, in	77,120	1 31,5	2 5,3
η Bootis May 4, 1691, in	77,071	1 31,8	2 10,3
η Bootis May 27, 1692, in	75,978	1 28,4	2 9,3
By π Bootis May 27, 1692, in	75,978	1 34,2	2 33,0
π Bootis Feb. 14, 1690, in	78,257	not obſ.	2 35,2

As the quantity of the motion of Arcturus ſouthward in declination, as deduced from a compariſon with η Bootis, differs conſiderably from the quantities given by the ſmall ſtar and π Bootis, which agree very nearly together, I have compared η Bootis with ſome of the neighbouring ſtars, as that ſtar, though of the third magnitude only, may have a ſmall motion of its own.

On the 14th of February, 1690, the difference of declination between η and π Bootis was obſerved by Mr. Flamſteed to be = 2°, 11′ 47″,8. By computation, that difference in 1768 ſhould have been 2′ 43″,9 leſs, = 2° 9′ 3″,9: but it was actually obſerved

obſerved to be 2° 8′ 34″,3 only. The ſtar η Bootis therefore appears by this compariſon to have moved ſouthward 29″,6 in 78,257 years.

On the 27th of May, 1692, η Bootis was obſerved by Mr. Flamſteed to be 2° 11′ 37″,8 to the north of π Bootis, which quantity ſhould by computation be 2′ 39″,1 leſs in 1768, or 2° 8′ 58″,7. But it was found to be 2° 8′ 34″,3. The ſtar η therefore appears to have moved ſouthward 0′ 24″,4 in 75,978 years.

On the 25th of April, 1693, η Bootis was obſerved to be 40′ 20″,8 to the ſouth of *f* Bootis, a ſtar of the 6th magnitude; and by myſelf that difference was obſerved to be 42′ 37″,5, by taking the mean of two obſervations on the 24th and 26th of May, 1768, differing only 4″,7. According to computation, the variation of η Bootis in declination during the interval of the two obſervations was 1359″,3, and of *f* Bootis 1256″,0; and therefore the difference of variation in declination was 1′ 43″,3, by which the diſtance of the ſtars was increaſed. The difference in declination therefore in 1768, if neither of the ſtars moved, ſhould have been 0° 42′ 04″,1; but it was obſerved to be 33″,4 greater, by which quantity therefore η Bootis muſt have moved ſouthward in 75,052 years.

By reducing all the foregoing deductions to 78 years, Arcturus appears to have moved,

		Weſtw.	Southw.
		′ ″	
By the ſmall Star,	Feb. 14, 1690	1 52,380	2 36,43
η Bootis	Feb. 14, 1690	1 18,541	2 5,88
η Bootis	Apr. 5, 1691	1 32,557	2 6,75
η Bootis	May 4, 1691	1 32,906	2 11,87
η Bootis	May 27, 1692	1 30,752	2 12,74
By π Bootis	May 27, 1692	1 36,707	2 37,07
π Bootis	Feb. 14, 1690	not obſerv.	2 34,69

But the Star η Bootis appears alſo to have moved Southward.

		′ ″
By π Bootis	Feb. 14, 1690	0 29,503
π Bootis	May 27, 1692	0 25,049
By *f* Bootis	Apr. 5, 1693	0 34,712
By a mean		0 29,755

As Arcturus appears to have moved ſouthward of η Bootis 2′ 9″,31, by taking a mean of the four quantities reſulting from the compariſons with that ſtar; and as η Bootis has alſo moved ſouthward of ſome of the neighbouring ſmall ſtars by 29″,755 in the ſame time, Arcturus upon the whole has moved 2′ 39″06 to the ſouth, by the compariſons with η Bootis only; and therefore, by taking a mean of all the reſults, Arcturus has altered its right aſcenſion leſs than the neighbouring ſtars by 1′ 33″,97 in 78 years, in which time it has alſo moved 2′ 36″,81 to the ſouth of the ſame ſtars.

In order to ſee how far the motion of right aſcenſion is to be depended upon, which is deduced from the above compariſons, I have ſelected and computed the following obſervations, made at Shirburn caſtle

caſtle with a tranſit inſtrument of $5\frac{1}{4}$ feet, placed exactly in the plane of the meridian, and conſequently more to be relied upon than thoſe made with a mural inſtrument.

By a mean of five obſervations, made on the 7th, 12th, 23d, 24th, and 31ſt of May, 1741, O. S. the difference in right aſcenſion between η Bootis and Arcturus was $5^\circ\ 22'\ 38'',9$, the extremes differing only $4'',4$ of a degree. The difference in the variation of right aſcenſion to the end of May, 1768, is $20'',5$, by which the aſcenſional difference is diminiſhed. It ſhould therefore have been $5^\circ\ 22'\ 18'',4$; but it was obſerved to be $5^\circ\ 21'\ 43'',4$. Therefore in 27 years Arcturus has moved weſtward $35'',0$.

On the 16th and 20th of May, 1744, the difference in right aſcenſion between η Bootis and Arcturus was $5^\circ\ 22'\ 30'',0$ by each of the obſervations, which difference ſhould have been, ſuppoſing neither of the ſtars to have any proper motion, $5^\circ\ 22'\ 11'',7$ in May, 1768. But it was found to be $28'',3$ leſs; by ſo much therefore had Arcturus moved weſtward in 24 years.

On the 24th of May, and 8th of June, 1746, the difference in right aſcenſion between the ſame ſtars was $5^\circ\ 22'\ 26'',2$, by taking a mean of the two obſervations; that difference ſhould have been $5^\circ\ 22'\ 09'',5$ in 1768. But it was obſerved $= 5^\circ\ 21'\ 43'',4$. Arcturus therefore in 22 years has moved $26'',1$ to the weſt.

Laſtly; on the 16th of April, and 27th and 28th of May, 1747, the difference in right aſcenſion between η Bootis and Arcturus, by taking a mean of the three obſervations, was $5^\circ\ 22'\ 25'',0$. By computation

putation the variation in the difference of right aſcenſion was 16″,0, by which the aſcenſional difference ſhould have been diminiſhed, and = 5° 22′ 09″,0. But by obſervation it was found = 5° 21′ 43″,4: Arcturus therefore by this laſt obſervation appears to have gone 25,6,″ weſtward.

By the obſervations therefore at Shirburn Caſtle Arcturus appears to have gone weſtward as in the following table; in the laſt column of which are contained the quantities reſulting from the obſervations of each year, reduced to 78 years.

	′ ″	′ ″
1741	0 35,0	1 41,11
1744	0 28,3	1 31,97
1746	0 26,1	1 32,59
1747	0 25,6	1 34,90
	Mean	1 35,14

The mean of all the obſervations, when reduced to an interval of 78 years, is 1′ 35″,14, which differs only 1″,17 from the mean of the other compariſons.

As then the proper motions of Arcturus weſtward in right aſcenſion = 1′ 33″,974, and 2′ 36″, 81 in declination ſouthward, ſeem well eſtabliſhed, the real motion of Arcturus is inclined in an angle of 30° 56′ to the weſt of the meridian or horary circle, and to be in that direction 3′ 2″,81 in 78 years, or at the rate of 2″,343 in a year. As this direction of its motion is nearly perpendicular to the plane of the ecliptic, the latitude of Arcturus muſt diminiſh yearly almoſt in the ſame proportion; and its longitude will alter leſs than that of other ſtars, though not ſo conſiderably as its right aſcenſion.

The

The proper motion of Arcturus then, in right aſcenſion Weſtward, being 1″,205, and in declination 2″,005, its annual preceſſion in right aſcenſion is 41″,108, and in declination 19″,133; and the true right aſcenſion of Arcturus, on Jan. 1, 1773, is 211° 19′ 47″,4, and declination North 20° 22′ 23″,3.

As none of the other principal ſtars have been found to have a motion ſo conſiderable as this, though many of the ſtars of the firſt magnitude, as for inſtance, Sirius, Procyon, α Aquilæ, α Orionis, as alſo β Aquilæ of inferior magnitude, do really vary their poſitions (and perhaps all of the firſt order will hereafter be found to have a proper motion), we may, I think, fairly conclude that Arcturus is the neareſt ſtar to our ſyſtem, viſible in this hemiſphere. If therefore the annual parallax of the fixed ſtars can ever be diſcovered, that is, if the diameter of the annual orbit bear a ſenſible proportion to the diſtance of the neareſt fixed ſtar, it is moſt likely to be diſcovered from the obſervations of Arcturus. The ſyſtem of the world, conſidered in an enlarged ſenſe, and agreeable to the idea we may entertain of an all-powerful benevolent Creator, may be taken to occupy the whole abyſs of ſpace, and to conſiſt of an aſſemblage of bodies, having different magnitudes, and emitting various degrees and modifications of light. The apparent change of ſituation viſible from the planet which we inhabit, and which revolves round one of the great bodies conſtituting a part of the general ſyſtem, as a center, may be owing either to the motion of our own ſyſtem in abſolute ſpace, or, if our ſyſtem ſhould be at reſt, to a real motion in

the ſtars themſelves: from whence the angular diſtances of the ſtars muſt vary in proportion to the velocity of thoſe motions, or to the direction of thoſe motions with reſpect to ourſelves. I have reaſon, at preſent, to believe that a ſmall motion may be diſcovered in the ſtar *o* Ceti, and perhaps in other ſtars that vary in degrees of brightneſs, which the diligence of future aſtronomers will diſcover, and perhaps in leſs time than at firſt ſight might ſeem neceſſary, when we conſider the ſeveral improvements which have of late been made in the methods of obſerving the heavenly bodies.

As the motion of Arcturus in declination (the quantity of which we have thus endeavoured to aſcertain) has been often acknowledged, it is matter of wonder that ſome aſtronomers, by comparing either the altitude or zenith diſtance of the Sun's limb with Arcturus, without previouſly ſettling the quantity of that ſtar's motion in declination, or at leaſt doing it indirectly, ſhould endeavour to determine whether the obliquity of the ecliptic hath remained conſtant, or ſtill continues to diminiſh, as it ſhould ſeem to have done for many centuries paſt, from the obſervations of ſucceſſive aſtronomers. Mr. Caſſini, and Mr. le Monnier, have both practiſed this method, and are of opinion, that the obliquity of the ecliptic hath not altered; or, if it hath altered, that the quantity of its alteration is not near ſo conſiderable as hath been imagined by ſome celebrated aſtronomers. By obſerving for ſeveral days before and after the ſolſtice the altitude or zenith diſtance of the Sun's limb, and that of a ſtar ſituated near the ſame parallel, the differences to be remarked in proceſs of time

time in the diſtances of the Sun from that ſtar (the motion of the ſtar in declination being allowed for during that interval of time), will be the quantity by which the Sun will have approached to or have receded from the ſtar. If the ſtar were abſolutely a fixed point, and the obſervations ſufficiently numerous, that, by taking a mean, the neceſſary and unavoidable errors in obſervation might either be conſiderably diminiſhed or almoſt annihilated, the method might be practiſed to great advantage. But as the ſtar (Arcturus) had a proper motion, and its apparent place was continually varying from the effect of the nutation of the earth's axis; as the limb of the Sun was ſometimes approaching to, and ſometimes receding from, the ſtar, by a kind of libratory motion from the effect of the nutation; alſo and as the obliquity of the ecliptic itſelf was, in all probability, continually diminiſhing; from a combination and as it were involution of theſe motions no certain concluſion could be drawn, ſince, in the ſpace of a few years, the apparent obliquity may be the ſame, and yet the mean obliquity may have diminiſhed, or perhaps, in the ſpace of a few years, the obliquity may appear to have increaſed, when it may really have become leſs. Whereas, by reducing the obſervations to their mean poſition, and by aſſigning to each known cauſe its proper and allowed effect, a regularity and uniformity muſt neceſſarily take place, as far at leaſt as is conſiſtent with the unavoidable errors in obſerving.

Mr. Caſſini, in the Memoirs of the Academy of Sciences for 1767, acquaints us, that, in 1748, the apparent diſtance of Arcturus from the upper limb

of the Sun, at the time of the ſolſtice, was the ſame as in 1766.

	°	′	″	‴
In 1748. Diſtance of Arcturus from the ſolſtitial limb of the Sun	3	13	36	40
Altitude of Arcturus	61	41	17	0
Therefore the apparent ſolſtitial altitude	64	55	13	40
In 1766. Diſtance of Arcturus from the ſolſtitial limb of the Sun	3	19	32	0
Altitude of Arcturus	61	35	42	0
Therefore the apparent ſolſtitial altitude	64	55	14	0

The ſame aſtronomer has, in the Memoirs for 1759, p. 325, communicated the following concluſions.

		Diſt. of the ſtar from ☉'s limb.			Reduction.			Solſtitial diſtance.		
1763.		°	′	″		′	″	°	′	″
June	14.	3	7	29	+	11	1	3	18	30
	15.	3	10	16	+	8	13	3	18	29
	25.	3	15	40	+	2	48	3	18	28
July	1.	2	59	1	+	19	22	3	18	23
	2.	2	54	55	+	23	33	3	18	28
	3.	2	50	18	+	28	8	3	18	26
							Mean	3	18	27

Mr. le Monnier, in the Memoirs for 1762, p. 269, has publiſhed the following diſtances of Arcturus from the limb of the Sun, reduced to the ſolſtitial point, with a view to obtain differences in the apparent obliquity of the ecliptic: and, from the obſervations

vations made with the gnomon of St. Sulpice, and communicated by Mr. le Monnier, in the ſame volume, it ſhould ſeem that that aſtronomer is of opinion, that the obliquity of the ecliptic hath no other variation than what the nutation of the earth's axis will occaſion; and that therefore we muſt either abandon the abſolute diminution of the ecliptic, or at leaſt ſuppoſe it extremely ſmall, ſince, in the ſpace of eighteen years, it hath not produced a ſenſible alteration.

	°	′	″	
1738.	3	10	15	
1740.	3	11	5	
1742.	3	11	48	
1763.	3	18	40	with the mural quadrant of 5 feet.
	3	18	35	with the large mural inſtrument.

As the reſult of the obſervations only, and not the obſervations themſelves, are communicated, I have only to obſerve, that there is a very conſiderable difference between the concluſions of the two aſtronomers for the ſame year 1763, and, at the ſame time, to declare my ſuſpicion, that if the *apparent* (for ſuch I apprehend them to be) were reduced to the *mean* diſtances, they would probably afford a confirmation of the diminution of the ecliptic. For the following obſervations of the Sun's zenith diſtance, made at Shirburn-Caſtle, near the ſummer ſolſtices of the years 1743, 1746, 1748, and 1766, and of Arcturus in the years 1743, 1746, and 1766, when reduced to their mean ſtate at the ſolſtice, do not confirm the aſſertion of Mr. Caſſini, but are an evident

and

and abſolute proof that the obliquity of the ecliptic has ſenſibly diminiſhed during an interval of 23, and even of 18 years.

The obſervations of 1743 were made with a mural quadrant of five French feet, conſtructed by the late Mr. Siſſon: but as the linear diviſions were found to be ſomewhat leſs accurate than was expected, and as the body of the quadrant was not framed with proper ſtrength and ſolidity, Mr. Bird was employed in the ſummer of the year 1745, by the Earl of Macclesfield, (the body of the inſtrument having been ſtrengthened by ſcrewing a large and broad plate of braſs upon the croſs-bars), to put a ſet of points upon the limb between the 90 and 96 arches of linear diviſions. By theſe operations the line of collimation was found to have varied, and to be $= 6'',3$, by which the zenith diſtances were given too ſmall, by the poſitive diviſions, from the end of 1746 to the end of June 1751, when Mr. Bird biſected the ſpaces between the points which he had formerly added in 1745. But after the year 1751, the error of the line of collimation was $= 2'',6$, as appears from obſervations of γ Perſei, β and γ Draconis, by which the zenith diſtances are alſo given too ſmall; and in that ſtate the inſtrument continued to the year 1767, when a new ſet of wires was put into the teleſcope, and the line of collimation thereby altered. The error of the line of collimation from 1743 to 1745 cannot directly be aſcertained, for want of zenith obſervations; but, from ſome indirect methods, it ſhould ſeem that the error was as nearly as poſſible $= 2''$, to be added to the obſerved zenith diſtances.

1743.

		Observed zenith distance.			Barometer.	Thermometer.	Refraction.	Sun's semidiameter.	Dist. from solstice.	Observations reduced to solstice.
		°	′	″			″ +	′ ″	° ′ — ″	° ′ ″
1743.										
June	7.	Up. L.	28	37 39	29,54	62	30,2	+ 15 48,1	0 43 5,7	28 10 51,6
	8.	Lo. L.	29	3 37	29,36	60	30,8	— 15 48	37 22,3	28 10 57,5
	9.	Up. L.	28	26 44,2	29,46	59½	30	+ 15 47,9	32 3,3	28 10 58,8
	12.	Up. L.	28	13 14,5	29,72	61½	29,9	+ 15 47,6	18 31,6	28 11 0,4
	18.	Up. L.	27	57 9	29,81	67	29,4	+ 15 47,3	2 30	28 10 55,7
	19.	Lo. L.	28	27 28,5	29,54	67½	29,8	— 15 47,2	1 16,1	28 10 55
	21.	Up. L.	27	54 41,8	29,66	66	29,1	+ 15 47,1	0 2,8	28 10 55,2
	22.	Lo. L.	28	26 22,2	29,50	63	29,8	— 15 47	0 3,4	28 11 1,6
	23.	Up. L.	27	55 10,2	29,59	61	29,5	+ 15 47	0 28,6	28 10 58,1
	24.	Lo. L.	28	27 35,5	29,59	58	30,4	— 15 46,9	1 18,8	28 11 0,2
	25.	Up. L.	27	57 15,5	29,39	58	29,5	+ 15 46,9	2 33,7	28 10 58,2
	27.	Lo. L.	28	32 34	29,57	58	30,5	— 15 46,9	6 17,9	28 10 59,7
							Mean			28 10 58,2
							Sun's parallax			— 4,1
										28 10 54,1
							Nutation			+ 6,7
										28 11 0,8
							Line of collimation			+ 2
							Mean solstitial zenith distance, 1743,			28 11 2,8
1746.			°	′ ″			″	′ ″	° ′ ″	° ′ ″
May	31.	Lo. L.	29	57 50,5	29,48	78	30,5	— 15 48,8	1 31 41,9	28 10 50,3
June	5.	Up. L.	28	48 24,8	29,62½	64 —	30,4	+ 15 48,3	53 50	28 10 53,5
	6.	Lo. L.	29	13 36,3	29,62	65	30,8	— 15 48,2	47 26,3	28 10 52,6
	7.	Up. L.	28	35 58,9	29,53	71 +	29,4	+ 15 48,1	41 24,3	28 10 52,1
	10.	Lo. L.	28	52 1,8	29,28	67	29,8	— 15 47,8	25 49,8	28 10 54
	16.	Up. L.	28	0 14,4	28,69	59½	28,7	+ 15 47,3	5 34,6	28 10 55,8
	19.	Lo. L.	28	27 5,1	29,60	58½	30,3	— 15 47,2	0 59,8	28 10 48,4
	22.	Up. L.	27	54 45	29,75	63½	29,4	+ 15 47	0 8	28 10 53,4
	23.	Lo. L.	28	26 47,4	29 85	64	30,2	— 15 47	0 40,4	28 10 50,2
	26.	Up. L.	27	59 23,4	29,63	69	29	+ 15 46,9	4 45,9	28 10 53,4
	27.	Lo. L.	28	33 6,6	29,63	74½	29,2	— 15 46,9	6 57,1	28 10 51,8
	28.	Up. L.	28	4 11,8	29,50	65½	29,3	+ 15 46,9	9 32,9	28 10 55,1
	30.	Lo. L.	28	42 5,9	29,22	63½	30,2	— 15 46,9	15 58,2	28 10 51
July	4.	Up. L.	28	28 16,6	29,67	69	29,6	+ 15 46,9	33 40,1	28 10 53
							Mean			28 10 52,5
							Sun's parallax			— 4,1
										28 10 48,4
							Nutation			+ 9,4
										28 10 57,8
							Error of the line of collimation . .			+ 6,3
							Mean solstitial zenith distance, 1746,			28 11 4,1

	Observed zenith distance.	Barometer.	Thermometer.	Refraction.	Sun's semidiameter.	Dist. from solstice.	Observations reduced to solstice.
1748.	° ′ ″			″ +	′ ″	° ′ ″	° ′ ″
June 15.	Up. L. 28 1 21,1	29,51½	64	29,3	+ 15 47,4	0 6 38,6	28 10 59,2
16.	Lo. L. 28 30 41,3	29,55	60	30,3	— 15 47,4	4 31,9	28 10 52,3
20.	Up. L. 27 54 45,7	29,86½	63½	29,5	+ 15 47,2	0 5,7	28 10 56,7
21.	Lo. L. 28 26 11,3	29,65½	75 +	29,2	— 15 47,1	0 1,3	28 10 52,1
22.	Up. L. 27 55 3,5	29,67	81½	28,2	+ 15 47	0 21,4	28 10 57,3
23.	Lo. L. 28 27 18,7	29,60½	72 —	29,3	— 15 47	1 6,7	28 10 54,3
24.	Up. L. 27 56 59,7	29,55+	65	29	+ 15 46,9	2 16,7	28 10 58,9
29.	Lo. L. 28 40 29,3	29,90	64	30,5	— 13 46,9	14 18,7	28 10 55,2
						Mean	28 10 55,8
						Sun's parallax	— 4,1
							28 10 51,7
						Nutation	+ 6,1
							28 10 57,8
						Error of the line of collimation . .	+ 6,3
						Mean solstitial zenith distance, 1748,	28 11 4,1

	Observed zenith distance.	Barometer.	Thermometer.	Refraction.	Sun's semidiameter.	Dist. from solstice.	Observations reduced to solstice.
1766.	° ′ ″			″	′ ″	° ′ ″	° ′ ″
June 11.	Lo. L. 28 47 12	29,51	62 —	30,4	— 15 47,7	0 20 42,3	28 11 12,4
12.	Up. L. 28 11 38,3	29,38	61 —	29,6	+ 15 47,6	16 47,1	28 11 8,4
17.	Lo. L. 28 29 47,9	29,15½	66	29,4	— 15 47,3	3 20,2	28 11 9,3
21.	Up. L. 27 54 57,4	29,72	68¾	29	+ 15 47,1	0 29	28 11 13,5
22.	Lo. L. 28 26 39,6	29,87½	70¾	29,7	— 15 47	0 11,9	28 11 10,4
23.	Lo. L. 28 27 19,2	29,95½	75½	29,5	— 15 47	0 48,6	28 11 13,1
24	Up. L. 27 56 45,7	29,80	78¼	28,4	+ 15 46,9	1 50,1	28 11 10,9
25.	Lo. L. 28 29 41,2	29,49	74	29,2	— 15 46,9	3 16,2	28 11 7,3
27.	Up. L. 28 2 18,5	29,40	67	28,9	+ 15 46,9	7 22,6	28 11 11,7
29.	Lo. L. 28 29 30,6	29,28	66	29,7	— 15 46,9	13 7	28 11 6,4
						Mean	28 11 10,5
						Sun's parallax	— 4,1
							28 11 6,4
						Nutation	+ 7,6
							28 11 14
						Error of the line of collimation . .	+ 2,6
						Mean solstitial zenith distance, 1766,	28 11 16,6

	Observed zenith dist. of Arcturus.	Refraction.	Aberration.	Nutation.	Precession.	Observations reduced.
1743.	° ′ ″	″	″	″	″	° ′ ″
May 12.	31 6 57,8	34	— 1,9	+ 1,4	+ 2,1	31 7 33,4
17.	31 6 57,8	34	— 0,8	+ 1,4	+ 1,8	31 7 34,2
18.	31 6 57,8	34	— 0,6	+ 1,4	+ 1,8	31 7 34,4
June 5.	31 6 52,3	34	+ 2,9	+ 1,2	+ 0,9	31 7 31,3
9.	31 6 50,3	34	+ 3,8	+ 1,2	+ 0,7	31 7 30
10.	31 6 52	34	+ 4	+ 1,2	+ 0,6	31 7 31,8
16.	31 6 57	34	+ 5,3	+ 1,2	+ 0,3	31 7 37,8
18.	31 6 54,8	34	+ 5,5	+ 1,2	+ 0,1	31 7 35,6
25.	31 6 52	34	+ 6,7	+ 1,2	— 0,2	31 7 33,7
July 1.	31 6 52	34	+ 7,8	+ 1,2	— 0,5	31 7 34,5
Mean						31 7 33,6
Error of the line of collimation						+ 2
Mean zenith distance of Arcturus, June 21, 1743, . .						31 7 35,6
Mean zenith distance of the Sun's center, June 21, 1743,						28 11 2,8
Mean distance of Arcturus from the Sun's center, 1743,						2 56 32,8

	Observed zenith dist. of Arcturus.	Refraction.	Aberration.	Nutation.	Precession.	Observations reduced.
1746.	° ′ ″	″	″	″	″	° ′ ″
June 4.	31 7 51,1	34	+ 2,5	— 5,8	+ 1,1	31 8 22,9
21.	31 7 53,1	34	+ 6,1	— 5,9	0	31 8 27,3
22.	31 7 50,2	34	+ 6,3	— 5,9	0	31 8 24,6
23.	31 7 52,3	34	+ 6,4	— 5,9	— 0,1	31 8 27,7
Oct. 9.	31 7 58,6	34	+ 8,9	— 6,4	— 5,7	31 8 29,4
Mean						31 8 26,4
Error of the line of collimation						+ 6,3
Mean zenith distance of Arcturus, June 21, 1746, . .						31 8 32,7
Mean zenith distance of the Sun's center, June 21, 1746,						28 11 4,1
Mean distance of Arcturus from the Sun's center, 1746,						2 57 28,6

	Obſerved zenith diſt. of Arcturus.	Barometer.	Thermometer.	Refraction.	Aberration.	Nutation.	Preceſſion.	Obſervations reduced.
1766.	° ′ ″			″	″	″	″	° ′ ″
May 13.	31 14 20,7	29,46	43	34,9	−1,9	−7,6	+2	31 14 48,1
21.	31 14 23,1	29,81	46	35,1	−0,2	−7,6	+1,7	31 14 52,1
22.	31 14 19,9	29,67	49	34,7	0	−7,6	+1,6	31 14 48,6
June 23.	31 14 20,8	29,90	70	33,1	+6,4	−7,7	−0,1	31 14 52,5
Mean								31 14 50,3
Error of the line of collimation								+ 2,6
Mean zenith diſtance of Arcturus, June 21, 1766,								31 14 52,9
Mean zenith diſtance of the Sun's center, June 21, 1766,								28 11 16,6
Mean diſtance of Arcturus from the Sun's center, 1766,								3 3 36,3

From the foregoing obſervations, it appears that the mean ſolſtitial zenith diſtance in ſummer was as follows.

				Variation in 100 years.
	°	′	″	″
1743.	28	11	2,8	60
1746.	28	11	4,1	62,5
1748.	28	11	4,1	69,4
1766.	28	11	16,6	

And, by comparing the three former with the latter, the variation of the obliquity of the ecliptic in 100 years is as is expreſſed in the laſt column of the table.

By comparing the diſtance of Arcturus from the center of the Sun in 1743, with the ſame diſtance as obſerved in 1766 (an allowance being made for the proper motion of the ſtar during the interval, as alſo for its variation in declination ariſing from the preceſſion

ceſſion of the equinoxes), it appears that its diſtance is 17″,3 leſs than it would have been, if the diſtance of the Sun's center from the equator had remained unvaried. By that quantity, therefore, the obliquity of the ecliptic has altered in 23 years; which is at the rate of 75″,2 in 100 years.

By comparing, in like manner, the diſtance in 1746, the obliquity of the ecliptic has diminiſhed 15″,6 in 20 years, or 78″ in 100 years.

	°	′	″		°	′	″
Diſtance in 1743	2	56	32,8	In 1746.	2	57	28,6
Motion of the ſtar in decl. Southward		7	20,8			6	23,3
Computed diſtance in 1766 . .	3	3	53,6		3	3	51,9
Obſerved diſtance in 1766 . . .	3	3	36,3		3	3	36,3
Variation of obliquity			17,3				15,6

The foregoing deductions prove, I think, beyond all doubt, that the obliquity has become leſs; but as the interval of time between the two terms of compariſon is ſo ſhort, that the errors committed in obſerving may bear a ſenſible proportion to the ſmall quantities juſt now found, and which, perhaps, are ſomewhat too large; let us have recourſe to Mr. Flamſteed's obſervations, and compare them with obſervations made by myſelf, in the courſe of the laſt and preſent years. For this purpoſe, I have reduced all the obſervations of the Sun, made in 1690, from May 26 to June 24, O. S. and alſo all the obſervations of Arcturus, made in the ſame year, to their mean poſition at the ſummer ſolſtice of that year. The obſervations, together with my own made at Oxford, are as follows.

1690.	Observed zen. dist. of the Sun's limbs.				Refraction.	Sun's semidiameter.		Dist. from solstice.			Observations reduced.		
		°	′	″	″	′	″	°	′	″	°	′	″
May 26.	Lo. L.	29	6	20	31,8	15	46,4	0	50	26,1	28	0	39,3
	Up. L.	28	34	50	31,1	15	46,4						41,4
June 2.	Lo. L.	28	31	35	31,1	15	45,7	0	15	28,3			52,1
	Up. L.	28	0	5	30,4	15	45,7						52,8
3.	Lo. L.	28	28	20	31	15	45,6	0	12	8,6			56,8
	Up. L.	27	56	45	30,3	15	45,6						51,1
4.	Lo. L.	28	25	15	30,9	15	45,5	0	9	10,9			49,5
	Up. L.	27	53	55	30,3						28	0	59,9
6.	Lo. L.	28	20	40	30,8	15	45,5	0	4	30,1	28	0	55,2
	Up. L.	27	49	15	30,2						28	1	0,6
7.	Up. L.	27	47	10	30,1	15	45,4	0	2	46,8	28	0	38,7
	Lo. L.	28	18	40	30,8						28	0	38,6
10.	Lo. L.	28	16	10	30,7	15	45,3	0	0	5,5	28	0	49,9
	Up. L.	27	44	40	30,1						28	0	49,9
12.	Up. L.	27	44	50	30,1	15	45,3	0	0	22	28	0	43,4
	Lo. L.	28	16	45	30,8						28	1	8,5
13.	Lo. L.	28	17	17,5	30,8	15	45,1	0	1	7,4	28	0	55,8
	Up. L.	27	45	50	30,1						28	0	58,8
14.	Up. L.	28	13	30	30,8	15	45,1	0	2	17,6	28	0	58,1
	Lo. L.	27	46	50	30,1						28	0	47,6
16.	Up. L.	27	50	40	30,2	15	45,1	0	3	51,8	28	1	3,5
	Lo. L.	28	22	15	30,9						28	1	9
17.	Up. L.	27	53	5	30,2	15	45	0	8	16,4	28	1	3,8
	Lo. L.	28	24	30	30,9						28	0	59,5
20.	Lo. L.	28	34	15	31,1	15	45	0	17	56,4	28	1	4,7
	Up. L.	28	2	45	30,4						28	1	4
24.	Up. L.	28	21	5	30,8	15	45,1	0	36	30,6	28	0	50,3

Mean		28	0 54,2
Error of the line of collimation			1 30
		27	59 24,2
Sun's parallax			— 4,1
		27	59 20,1
Nutation			+ 9,5
Mean solstitial zen. dist. of the Sun's center, June 11, 1690, O. S.		27	59 29,6

	Observed zenith dist. of Arcturus.	Refraction.	Aberration.	Nutation.	Preceſſion.	Obſervations reduced.
1690.	° ′ ″	″	″	″	″	° ′ ″
Feb. 14.	30 39 20	33,8	—12,2	— 5,1	— 2,3	30 39 34,2
Apr. 13.	30 39 20	33,8	— 5,5	— 5,3	— 5,4	30 39 37,6
25.	30 39 10		— 3,3	— 5,4	— 6	29,1
26.	30 39 15		— 2,9	— 5,4	— 6,1	34,4
May 13.	10		+ 0,4	— 5,5	— 6,9	31,8
14.	10		+ 0,6	— 5,5	— 7	31,9
15.	30 39 10	33,8	+ 0,8	— 5,5	— 7	32,1
20.	10		+ 1,9	— 5,5	— 7,3	32,9
22.	10		+ 2,3	— 5,5	— 7,4	33,2
24.	30 39 5		+ 2,7	— 5,6	— 7,5	28,4
June 12.	10	33,8	+ 6,4	— 5,6	— 8,5	36,1
13.	10		+ 6,6	— 5,6	— 8,6	36,2
16.	30 39 10		+ 7,1	— 5,6	— 8,7	36,6
17.	10		+ 7,2	— 5,6	— 8,8	30 39 33,4
July 1.	30 39 10	33,8	+ 9,4	— 5,7	— 9,5	30 39 30,6
Dec. 13.	30 39 35	33,8	— 6,6	— 6,5	—18,1	30 39 37,6
14.	30 39 40	33,8	— 6,8	— 6,5	—18,2	30 39 42,3

Mean, January 1, 1690, O. S.	30 39 34
Preceſſion to June 11, 1690	+ 8,4
Mean zenith diſtance of Arcturus, June 11, 1690 . .	30 39 42,4
Mean ſolſtitial zenith diſtance of the Sun's center, June 11, 1690	27 59 29,6
Mean diſtance of Arcturus in declination from the Sun's center, 1690	2 40 12,8

	Observed zenith distance of the Sun's upper limb.	Barometer.	Thermometer.	Refraction.	Sun's semidiameter.	Dist. from solstice.	Observations reduced.
1771.	° ′ ″			″	′ ″	° ′ ″	° ′ ″
June 8.	28 36 47,6	30,08½	64⅓	30,9	15 48	0 36 0,4	28 17 6,1
14.	28 11 38,1	29,94	63 —	30	15 47,4	10 46,6	8,9
18.	28 3 4,4	29,75	61 +	29,7	15 47,2	2 8,3	13
21.	28 0 55,4	30,08⅔	65⅓	29,7	15 47	0 0,8	11,3
22.	28 1 1,4	30,07	66 +	29,6	15 47	0 7,6	28 17 10,4
24.	28 2 26,1	30,07	62	30	15 46,9	1 35,8	7,2
25.	28 3 46,9	30,05	09	29,5	15 46,9	2 56,8	6,5
26.	28 5 38,1	29,97½	68	29,5	15 46,9	4 42,7	11,8
29.	28 13 21,3	29,84	61 —	30,2	15 46,9	12 27,7	28 17 10,7

Mean	28 17 8,7
Sun's parallax	— 4,1
	28 17 4,6
Nutation	— 6,8
	28 16 57,8
Error of the line of collimation	+ 4,8
Mean solstitial zenith distance of the Sun's center, 1771,	28 17 2,6

1772.

	Observed zenith distance of the Sun's limbs.			Barometer.	Thermometer.	Refraction.	Sun's semidiameter.		Dist. from solstice.			Observations reduced.		
		°	′	″			″		°	′	″	°	′	″
1772.														
June 8.	Lo. L.	29	4	31,3	30,18	71	30,7	−15 48	0	31	59,9	28	17	14,1
9.	Up. L.	28	28	1	30,05¼	67½	30,1	+15 47,9		27	5,4	21	17	13,6
11.	Lo. L.	28	51	5,5	30,10½	71¾	30,3	−15 47,7		18	29,2			18,9
12.	Lo. L.	28	47	15,7	30,22⅔	60⅔	31,2	−15 47,6		14	48,4			10,9
14.	Up. L.	28	9	34,4	30,15½	63	30,1	+15 47,4		8	38,4			13,5
	Lo. L.	28	41	11,1			30,8							16,1
15.	Up. L.	28	7	5,4	29,94½	72	29,2	+15 47,3		6	10,7			11,2
	Lo. L.	28	38	40,8			29,8					28	17	12,6
16.	Up. L.	28	5	0,3	29,82½	70¾	29,1	+15 47,3		4	7,6			9,1
	Lo. L.	28	36	38,6			29,8							13,5
18.	Up. L.	28	2	16,4	30,02¾	71½	29,2	+15 47,2		1	15,6			17,2
	Lo. L.	28	33	48,1			29,7							15
19.	Up. L.	28	1	26,3	29,97	75⅓	28,8	+15 47,1		0	26,8			15,4
	Lo. L.	28	32	55,8			29,5							11,4
20.	Lo. L.	28	32	30,3	29,93	79	29,1	−15 47,1		0	2,8	28	17	9,5
21.	Lo L.	28	32	32,8	29,79	72	29,6	−15 47		0	3,5			11,9
22.	Up. L.	28	1	24,8	30,00¼	73	29	+15 47		0	29,9	28	17	10,9
23.	Up. L.	28	2	14,4	30,01	76¼	28,8	+15 47		1	19,4	28	17	10,8
	Lo. L.	28	33	54,1			29,5							17,2
26.	Up. L.	28	7	20,3	29,96	85 −	28,2	+15 46,9		6	18,9			16,5
	Lo. L.	28	38	50,4			28,8					28	17	13,4

Mean		28	17 13,4
Sun's parallax			− 4,1
		28	17 9,3
Nutation			− 8,7
		28	17 0,6
Error of the line of collimation			+ 4,8
Mean solstitial zenith distance of the Sun's center, 1772,		28	17 5,4

	Observed zenith distance of Arcturus.	Barometer.	Thermometer.	Refraction.	Aberration.	Nutation.	Preceſſion.	Obſervations reduced.
1772.	° ′ ″			″	″	″	″	° ′ ″
July 11.	31 21 53,6	29,87	73	32,9	+ 9,4	+ 1,9	— 10,1	31 22 27,7
Aug. 11.	31 21 56,6	29,91	64 —	33,8	+ 12,2	+ 2,1	— 11,7	33
17.	50,6	29,93	75 +	32,8	+ 12,2	+ 2,1	— 12	25,7
19.	54,6	29,76	77 —	32,5	+ 12,3	+ 2,2	— 12,1	29,5
Oct. 16.	31 21 58,1	29,99¼	61	34,2	+ 7,7	+ 2,6	— 15,1	27,2
Nov. 2.	31 22 3,3	29,32¼	49 —	34,5	+ 4,4	+ 2,7	— 16	28,9
7.	22 3,5	29,25	49	34,3	+ 3,8	+ 2,7	— 16,3	28
12.	8,3	29,82	38	35,9	+ 2,1	+ 2,7	— 16,5	32,5
20.	31 22 12,3	29,85	46 +	34,1	+ 0,4	+ 2,7	— 16,9	31 22 32,6
21.	10,3	29,50 +	40 —	35,4	+ 0,2	+ 2,7	— 17	31,6
23.	11,3	29,37	40	35,2	— 0,2	+ 2,7	— 17,2	31,8
Dec. 10.	15,3	29,62	39½	35,7	— 3,9	+ 2,9	— 18	32
16.	31 22 12,3	29,69	45¼	35,1	— 5	+ 2,9	— 18,3	31 22 27

Mean zenith diſtance of Arcturus, January 1, 1772			31 22 29,8
Preceſſion to June 21, 1772			+ 9
Mean zenith diſtance of Arcturus, June 21, 1772			31 22 38,8
Error of the line of collimation			+ 4,2
True mean zenith diſtance of Arcturus, June 21, 1772			31 22 43
Mean zenith diſtance of the Sun's center, June 21, 1772			28 17 5,4
Mean diſtance of Arcturus in declination from the Sun's center, June 21, 1772			3 5 37,6
	° ′ ″		
Mean diſtance in June 1690 . .	2 40 12,8		
Preceſſion, &c. to June 1772 . .	26 16,4		
Computed diſtance in June 1772 .	3 6 29,2		
Obſerved diſtance in June 1772 .	3 5 37,6		
Diminution of obliquity in 82 years	51,6		

From

From the foregoing obſervations it appears that, at the ſummer ſolſtice of the year 1690, Arcturus was 2° 40′ 12″,8 to the South of the Sun's center in declination: the motion of the ſtar, in declination, from that time to the ſummer ſolſtice of the year 1772, including its proper motion, is 26′ 16″,4. Arcturus, therefore, in 1772, ſhould have been 3° 6′ 29″,2 to the South of the Sun's center, if the angle of the ecliptic and equator had not varied: but that diſtance was found by actual obſervation to be 51″,6 leſs. By ſo much therefore muſt the obliquity of the ecliptic have become leſs in an interval of 82 years; and, conſequently, the variation in 100 years will be 62″,92.

If the obſervations of Arcturus be reduced to the ſolſtice of 1771, and the zenith diſtance of the Sun's center, as obſerved in that year, be made uſe of in the ſame manner, the variation of the obliquity in 81 years will be found = 48″,8, and in 100 years = 60″.

If the quantity of the arc of Mr. Flamſteed's inſtrument were accurately known, the obſervations which he made at the winter ſolſtice in 1690 might be compared with later obſervations, in order to determine both the quantity of the obliquity in 1690, and alſo the variation ſince his time. Accordingly, I have endeavoured to determine the error of the arc of the inſtrument between 28° and 75° of zenith diſtance, and proceeded in the following manner. I computed ſeveral obſervations of the ſtars ζ Tauri, η Pleïadum, η and μ Geminorum, and φ, σ, and ο Sagittarii, as obſerved by Mr. Flamſteed, in the years 1690, 1691, and 1692, and reducing them

to the years 1760 and 1766, I compared the differences of declination between thoſe ſtars, reſulting from Mr. Flamſteed's obſervations, with the differences given by the places of the ſame ſtars, as ſettled by Dr. Bradley in 1760, and alſo by actual obſervations of the ſame ſtars made at Shirburn-Caſtle in 1766; and, by combining theſe differences together, I found that the whole arc of 90 degrees was too ſhort by 43″. Suppoſing the error to be uniform, the proportional part of this quantity, thus found for the ſolſtitial zenith diſtance of the Sun in June = 13″,4, is nearly confirmed upon the authority of Mr. Flamſteed himſelf, who, in the prolegomena to the third volume of the Hiſtoria Cœleſtis, where he is deducing the latitude of the Royal Obſervatory at Greenwich, and the quantity of the obliquity in 1690, from his own obſervations, allows the zenith diſtances at 28°, 36°, and 40°, on his inſtrument, to be too ſmall by 15′ and by 20″, at 75°.

I have therefore computed the obſervations of the Sun, made from November 30 to December 20 of 1690, which, reduced to the ſolſtice, are as in the following table; to which are ſubjoined the obſervations made by myſelf at Oxford, at the winter ſolſtice of 1771.

	Observed zenith distance of the Sun's limbs.			Refraction. +		Sun's semidiameter.		Dist. from solstice. +			Observations reduced.		
1690.		°	′	″	′	″	′	″	°	′	″	°	′	″
Nov. 30.	Lo. L.	74	45	17,5	3	27,9	— 16	16,6	0	25	43,1	74	58	11,9
	Up. L.	74	12	40	3	19,6	+ 16	16,6				74	57	59,3
Dec. 2.	Lo. L.	74	54	5	3	28,7	— 16	16,6		17	16,1	74	58	33,2
2.	Up. L.	74	21	25	3	21,7								19,4
10.	Lo. L.	75	11	0	3	32,7	— 16	17,2		0	4,6			20,1
	Up. L.	74	38	35	3	25,4								22,2
13.	Lo. L.	75	9	50	3	32,4	— 16	17,2		1	24,4	74	58	29,6
	Up. L.	74	37	15	3	25,3								21,9
15.	Lo. L.	75	6	45	3	31,6	— 16	17,3		4	39,4	74	58	38,7
	Up. L.	74	34	5	3	24,5								26,2
17.	Lo. L.	75	1	30	3	30,2	— 16	17,3		9	46,9			29,8
20.	Up. L.	74	13	15	3	19,8	+ 16	17,3		25	38,3	74	58	30,9
	Lo. L.	74	46	5	3	27	— 16	17,3				74	58	53,5

Mean	74	58	25,9
Error of the line of collimation		— 1	10
	74	57	15,9
Sun's parallax		—	8,5
	74	57	7,4
Nutation		—	9,6
Mean solstitial zenith distance of the Sun's center, Dec. 1690,	74	56	57,8

	Obſerved zenith diſtance of ⊙'s upp. limb.	Barometer.	Thermometer.	Refraction.	Sun's ſemidiameter. +	Diſt. from ſolſtice. +	Obſervations reduced.
1771.	° ′ ″			′ ″	′ ″	° ′ ″	° ′ ″
Dec. 8.	74 11 1,7	29,43	$44\frac{1}{4}$	3 21	16 18	0 42 26,3	75 13 12,2
12.	74 32 25,4	$29,56\frac{1}{3}$	47 +	3 25,2	16 18,5	21 3,4	17,7
16.	74 46 32,3	28,68	$45\frac{2}{3}$	3 22,7	16 18,7	7 2,5	21,8
19.	74 51 57,3	29,23	42	3 29,6	16 19	1 26,1	17,2
24.	74 51 48,5	29,07	40	3 29,6	16 19,1	1 31	13,4
25.	74 50 24,2	29,43	$41\frac{1}{4}$	3 31,2	16 19,2	2 56,9	16,7
27.	74 46 13,3	$29,42\frac{1}{2}$	$48\frac{1}{2}$	3 26,6	16 19,2	7 13,4	17,7
30.	74 36 13,9	30,13	$39\frac{1}{4}$	3 34,1	16 19,3	17 9,3	75 13 21,8
Mean							75 13 17,3
Sun's parallax							— 8,5
							75 13 8,8
Nutation							+ 7,9
							75 13 16,7
Error of the line of collimation							+ 4,8
Mean ſolſtitial zenith diſtance of the Sun's center, December 1771,							75 13 21,5

The mean obliquity of the ecliptic reſulting from the zenith diſtances, as obſerved at the two ſolſtices in 1690, by applying the known latitude of the place, will be found to be widely different, if no correction be applied for the error of the inſtrument.

	° ′ ″		° ′ ″
June, zenith diſtance	—27 59 29,6	Dec. zen. diſt.	74 56 57,8
Latitude of Greenwich	51 28 38		—51 28 38
	23 29 8,4		23 28 19,8

But if the obſervations be corrected by the error of the inſtrument, the two reſults will be found to agree together as nearly as can be expected.

°	′	″
27	59	29,6
	+	13,4
—27	59	43
51	28	38
Obliquity 23	28	55

	′	″
74	56	57,8
	+	35,8
74	57	33,6
—51	28	38
23	28	55,6

Or, if the obliquity be required independent of a knowledge of the latitude of the place, it will be found to be = 23° 28′ 55″,3.

	°	′	″
December	74	57	33,6
June	—27	59	43
Difference	46	57	50,6
Mean obliquity 1690, ½ Diff.	23	28	55,3

By commparing the obſervations at the ſummer ſolſtices of 1771 and 1772 with thoſe at the winter ſolſtice of 1771, it appears that the mean obliquity was about the beginning of the year 1772 = 23° 28′ 9″,4 and 23° 28′ 8″. I ſuppoſe therefore the mean obliquity to be 23° 28′ 8″ at the beginning of the preſent year; and conſequently, the obliquity has diminiſhed, by my obſervations, 47″ in 81 years, ſince Mr. Flamſteed's time, or at the rate of 58″ in 100 years, a quantity which will be found nearly at a mean of the computations framed by Mr. Euler and Mr. de la Lande, upon the principles of attraction.

Oxford, Dec. 23, 1772.

XV. *New*

XV. *New Obſervations upon Vegetation. By Mr.* Muſtel *of the Acad. of Sciences at* Rouen; *tranſlated from the* French.

Read Jan. 14, 1773. MANY celebrated writers, induced by the analogy, which they obſerved betwixt the vegetable and animal kingdoms, have admitted the circulation of the ſap in the one, in a ſimilar manner to the circulation of the blood in the other.

This important point of vegetable œconomy produced a diverſity of opinions, and has not yet been ſufficiently cleared up.

Dr. Hales, in his Vegetable Staticks, does not ſeem to embrace the ſyſtem of the circulation of the ſap; but he does not prove the contrary *.

* *Il ne prouve pas contre.* This certainly is a miſtake. Dr. Hales, in the IVth Chapter of his *Phyſical Staticks*, not only declares openly againſt the doctrine of the circulation of the ſap, and overturns the arguments alledged in favour of this opinion; but he produces ſeveral new experiments, which prove directly the impoſſibility of ſuch a circulation. (See p. 144, &c.) His reaſons have been thought ſo convincing, that the ſyſtem of the circulation in plants has been ever ſince exploded in England; and that they have had a ſimilar effect abroad, appears from the following quotation from a book of the ingenious Mr. Bonnet, F. R. S. of Geneva, intitled *Recherches ſur l'uſage des feuilles*, printed in 1754, p. 269. " Pour moi, perſuadé de la fauſſeté de " cette opinion (que la ſeve circuloit comme le ſang) par les ex- " périences de M. Hales (Ch. IV.) &c." M. M.

Mr.

Mr. Du-Hamel, in his Phyſiology of Trees, contents himſelf with relating what has been ſaid for or againſt this opinion; but, though he ſufficiently hints that he does not believe it true, he determines nothing about it. The friends of the circulation in plants have never been able to find in them any thing analogous to that powerful organ, which is the promoter of it in animals; for want of ſuch an organ, they were forced to imagine valves and paps in the lymphatick veſſels of plants, by means of which the liquors once introduced into the ſap veſſels were ſuppoſed to be hindered from going back; but, unfortunately, no body has ever been able to diſcover theſe valves and paps, ſo different from the ſimple contrivances, by which nature is uſed to arrive at her ends.

An experiment, which I made, and of which I propoſe giving an account in this paper, throws a great light upon this queſtion, as well as upon ſeveral others; and the concluſions deducible from it appear to me deciſive.

On the 12th of January, I placed ſeveral ſhrubs in pots againſt the windows of my hot-houſe, ſome within the houſe, and others without it. Through holes made for this purpoſe in the panes of glaſs, I paſſed a branch of each of the ſhrubs, ſo that thoſe on the inſide had a branch without, and thoſe on the outſide one within; after this, I took care that the holes ſhould be exactly cloſed and luted. This inverſe experiment, I thought, if followed cloſely, could not fail affording ſufficient points of compariſon, to trace out the differences, by the obſervation of the effects.

The

The 20th of January, a week after this diſpoſition, all the branches that were in the hot-houſe began to diſcloſe their buds. In the beginning of February, there appeared leaves, and towards the end of it, ſhoots of a conſiderable length, which preſented the young flowers. A dwarf apple-tree and ſeveral roſe-trees, being ſubmitted to the ſame experiment, ſhewed the ſame appearance then as they commonly put on in May; in ſhort, all the branches which were within the hot-houſe, and conſequently kept in the warm air, were green at the end of February, and had their ſhoots in great forwardneſs. Very different were thoſe parts of the ſame tree, which were without and expoſed to the cold. None of theſe gave the leaſt ſign of vegetation; and the froſt, which was intenſe at that time, broke a roſe pot placed on the outſide, and killed ſome of the branches of that very tree, which, on the inſide, was every day putting forth more and more ſhoots, leaves, and buds, ſo that it was in full vegetation on one ſide, whilſt frozen on the other.

The continuance of the froſt occaſioned no change in any of the internal branches. They all continued in a very briſk and verdant ſtate, as if they did not belong to the tree, which, on the outſide, appeared in the ſtate of the greateſt ſuffering. On the 15th of March, notwithſtanding the ſeverity of the ſeaſon, all was in full bloom. The apple-tree had its root, its ſtem and part of its branches, in the hot-houſe. Theſe branches were covered with leaves and flowers; but the branches of the ſame tree, which were carried to the outſide, and expoſed to the cold air, did not in the leaſt

partake

partake of the activity of the reſt, but were abſolutely in the ſame ſtate, which all trees are in during winter. A roſe tree, in the ſame poſition, ſhewed long ſhoots with leaves and buds; it had even ſhot a vigorous branch upon its ſtalk, whilſt a branch which paſſed through, to the outſide, had not begun to produce any thing, but was in the ſame ſtate with other roſe-trees left in the ground. This branch is four lines in diameter, and eighteen inches high.

The roſe-tree on the outſide was in the ſame ſtate; but one of its branches drawn through to the inſide of the hot-houſe, was covered with leaves and roſe-buds. It was not without aſtoniſhment that I ſaw this branch ſhoot as briſkly as the roſe-tree which was in the hot-houſe, whoſe roots and ſtalk, expoſed as they were to the warm air, ought, it ſhould ſeem, to have made it get forwarder than a branch belonging to a tree, whoſe roots, trunk, and all its other branches were at the very time froſt-nipt. Notwithſtanding this, the branch did not ſeem affected by the ſtate of its trunk; but the action of the heat upon it produced the ſame effect as if the whole tree had been in the hot-houſe.

It would be uſeleſs to give an account of the diary I kept throughout the courſe of this intereſting experiment. It may be ſufficient to obſerve, that the walk of nature was uniformly the ſame. The interior branches continued their productions in a regular manner, and the external ones began theirs at the ſame time, and in the ſame manner, as they would have done, had they been left in the ground. The fruits of the in-

terior branches of the apple-tree were, in the beginning of May, of the ſize of nutmegs; whilſt the bloſſoms but juſt began to ſhew themſelves on the branches without. I ſhewed Mr. Du-Tillet, of the Academy of Sciences at Paris, on his paſſage through this town, the effects of my experiments, and likewiſe communicated to him another obſervation, which chance occaſioned, and ought not to be omitted.

I obſerved that three of the flower buds of the apple-tree had been gnawed off by a ſnail in ſuch a manner, that all the petals and ſtamens had diſappeared, being eat up cloſe to the calyx. This not having been entered by the ſnail, the baſis of the piſtillum and the embryo were preſerved.

I took it for granted that theſe flowers would bear nothing; but I was ſoon convinced of my miſtake. Almoſt all of them bore fruit; the apples were perfectly formed, and ſix or ſeven pretty large ones too were ſeen upon each bunch. On the other hand, the ſnail had ſpared ſome other bunches, (doubtleſs becauſe more difficult to be got at;) but out of ten or twelve flowers in each bunch, not above one or two ſhewed any ſigns of fruit. This ſuggeſted to me the idea, that, when the flowers of trees are full blown, the prevention of the natural fall of the petals and ſtamens gives a greater aſſurance of the fructification; and on ſeveral times repeating the following experiment, I convinced myſelf that it did ſo. In imitation of the ſnail, I cut with my ſciſſars the petals of apple, pear, plumb, and cherry bloſſoms, cloſe to the calyx. Almoſt every one of thoſe, which

which were thus cut, ſucceeded, whilſt ſeveral of the neighbouring flowers miſ-carried.

Thus did a ſnail teach me how to render a tree fruitful; nor is it the firſt time that animals have been the inſtructors of mankind. I confeſs, however, that this proceſs is not very practicable in a large orchard: but it might be adopted in an eſpalier; in which one would chuſe to procure a great deal of fruit from trees of the beſt ſort. It may indeed be queſtioned, whether the ſuppreſſion of the ſtamens would not render the fruit barren; and in fact I found, that, though the flowers of the dwarf apple-tree, whoſe petals and ſtamens were eat up by the ſnail, gave me apples equally large and beautiful, and that. when I came to open them, I found the capſules formed as uſual at the center of them; yet they were entirely empty, without the leaſt appearance of a pip. Abſolute fructification conſequently did not take place; ſince botaniſts, with reaſon, call nothing fruit but the ſeed, which contains the germen, which is to perpetuate the ſpecies. All the other parts, being only intended to co-operate in the formation and preſervation of the ſeeds, periſh of courſe, when once the ſeeds are come to maturity and perfection, and the work of nature fulfilled.

Another remarkable thing in theſe apples is, that in the upper part there was found a much deeper cavity than uſual. It was eight or nine lines deep. The orifice of this cavity was bordered by five tubercles, indented and ſomewhat elevated; but there was no veſtige of the calyx, which, it is well known, remains always to the

upper part of apples and pears, and is commonly called the eye.

I now return to my firſt experiment; the conſequences of which, as I have deſcribed them, ſeem to prove,

I. Firſt that the circulation of the ſap does not take place in plants, as the circulation of the blood in animals. This may be deduced from the following obſervations.

The tree in the hot-houſe went through all its changes during the winter, and the branch expoſed to the open air underwent none; conſequently the ſap, which was in action in the root, ſtock, and head, of the tree, did not circulate through the branch without; which had no ſhare in the vegetation of the roots and trunk. It might, indeed, be argued that the cold air, to which this branch was expoſed, ſtopped the circulation, and therefore that the firſt experiment would not be deciſive; but the inverſe of it ſeems fully ſo.

The tree placed on the outſide of the hot-houſe continued, during the whole winter, in the ſtate of numbneſs, natural to all trees, which are expoſed at that ſeaſon; but one of its branches, which was in the hot-houſe, put forth ſucceſſively its buds, leaves, bloſſoms, and fruits. Whilſt therefore the root of the tree, to which this branch belonged, was in the ground ſo frozen, that the pot itſelf, in which it ſtood, was broken by it, whilſt the ſtock and top of the tree were ſo covered over with ice, that many of the branches were killed; this branch alone did not in the leaſt partake of the common ſtate of numbneſs and ſuffering, and was on the contrary in full vegetation.

tion. The ſap in it muſt have been extremely rarefied, and in very quick motion, whilſt that of the tree was greatly condenſed, and in total inaction. How is it poſſible to conceive a circulation of the ſap from ſuch a frozen root and ſtock, to a branch full of vigour, and loaded with leaves and flowers? Surely this experiment muſt appear concluſive againſt the ſyſtem of circulation; ſince in this caſe it could at beſt only be admitted to have taken place in the vegetating branch; and that would very improperly be termed circulation, which ſhould be confined to one limb.

II. This experiment proves, that each part of a tree is furniſhed with a ſufficient quantity of ſap to effect the firſt production of buds, flowers, and fruits. There is little probability that the branch drawn into the hot-houſe ſhould have derived its ſap from the roots of the tree: as they, at that time, lay in a very ſmall quantity of earth, rendered extremely hard and dry by the froſt, they could have but little liquor to ſpare; and even this, conſidering the congealed ſtate of the lymphatick veſſels of the ſtock, could have found no paſſage to the branch. This branch muſt of courſe have been enabled to continue its vegetation by the quantity of ſap with which it was provided, the conſumption of which muſt have been ſupplied at the firſt breaking of the froſt. This truth, now demonſtrable by experience, had been pointed out before by a multiplicity of other facts. Every body may have obſerved that a tree, which has been blown down in autumn, though ſeparated from its trunk, begins the ſame vegetation, that it would have done if it had remained ſtanding.

ing. Its buds open, it bears leaves, and even ſhoots, which ſometimes are very long, and muſt be the effects of the ſap it contained. It is true, indeed, that this appearance does not continue long, becauſe the proviſion of ſap once exhauſted, without being renewed, every thing muſt of neceſſity periſh.

An effect of the like kind often deceives us in trees, that have been newly planted, and in ſcions, which produce flowers and even fruits, without ever having taken root. But in this caſe the ſymptoms, which would ſeem to promiſe life, are on the contrary the forerunners of death; becauſe the leaves, being from their nature the moſt powerful organs of tranſpiration and diſſipation, the graft is the more readily exhauſted, when there is no root to furniſh it with a freſh ſupply of nutritive juices.

III. This experiment proves that it is heat, which unfolds the leaves, and produces the other parts of fructification, in the branch expoſed to its action.

Autumn is the time, in which Nature employs itſelf as it were clandeſtinely, under the cover of the leaves, in forming the buds, which contain the rudiments of the leaves, bloſſoms, and fruits, that are to be produced in the courſe of the ſucceeding ſummer. Theſe buds prepare and work themſelves out, during the winter, under the rough coats, that are deſtined to preſerve them from the injuries of the weather. As ſoon as the warm weather in the ſpring begins to be felt, the buds open, and their coats, which then become uſeleſs, drop off, and give place to the productions, which they

they contained and preſerved. Immediately after this, the bloſſoms, flowers and fruits make their appearance. This is the uſual operation; but in the caſe before us, nature was, as it were, ſurprized by art: what ſhe ſhould not have done till ſpring, ſhe did in the winter, becauſe the heat of the hot-houſe produced that expanſion, which, according to the natural courſe, ought to have been effected by the rays of the ſun darting leſs obliquely than before upon the horizon. There is no doubt but it is to heat, either natural or artificial, that this expanſion is owing; and the experiment proves that it is only in that part of the tree, which is expoſed to the effect of heat, that the ſap, which in every other part remains torpid and inactive, is put into motion, and produces vegetation. From this, it appears that the vegetable œconomy is different from the animal, and that thoſe, who endeavoured to eſtabliſh the circulation in both, carried their analogy too far.

This fact, now eſtabliſhed, furniſhes a good reaſon why in the tapping of the maple and ſugar-birch-trees, ſo much liquor runs out on one ſide, and none at all on the other. It is well known that, if during the time of a froſt, or a ſummer's day, towards noon, you bore a hole on the ſide of the maple-tree expoſed to the ſouth, you will get a great quantity of liquor from it; and that if you bore the north-ſide at the ſame time, you will not get a drop. The cauſe of this evidently appears from what has been ſaid. One likewiſe ſees why trees expoſed to the ſouth loſe a great many of their branches, and ſometimes die altogether, in the courſe of a ſevere winter; whilſt

trees of the ſame ſort, but placed to the north, or in ſome other expoſition, will ſtand the hardeſt froſts. This is particularly remarkable in the ever-greens, whoſe reſinous and oily ſap being liquefied by the heat of the ſun, the tree cannot eſcape ſuffering a great deal, whenever it is ſurprized in that ſtate by the night froſts. Thoſe obſervers, who attend to this, and know how well pines, firs, and bays ſucceed, when planted on the back of mountains expoſed to the north, will take care not to place ſuch kind of trees in a ſouthern aſpect, in hopes of their ſucceeding better by it.

Many other conſequences might be drawn from theſe experiments; but the bounds, I have aſſigned to this paper, do not allow it. I propoſe examining them more at large in a treatiſe upon vegetation, which, I hope, the obſervations and experiments I have made, may render intereſting and uſeful.

XVI. *Actual*

XVI. *Actual Fire and Detonation produced by the Contact of* Tin-foil, *with the Salt compoſed of Copper and the Nitrous Acid. By* B. Higgins, *M. D. Communicated by* R. Brockleſby, *M. D. F. R. S. and of the* Royal College *of* Phyſicians *in* London.

Read Dec. 24, 1772, and Jan. 28, 1773.

SEVERAL pieces of thin ſheet-copper, placed vertically, and at a ſmall diſtance from each other, in the ſtrong nitrous acid diluted with half its quantity, or more, of water, and ſuffered to remain in a cloſe veſſel, until the acid is ſaturated, afford a cryſtalline blueiſh green ſalt, which is to be ſeparated from the undiſſolved copper and the ſuperfluent green liquor, and kept in a well-corked bottle; becauſe, on expoſure to the air, it deliqueſces.

This ſalt, taken moiſt, but not very wet, and beaten to the fineneſs of baſket ſea-ſalt, in a mortar, is to be ſtrewed to the thickneſs of a ſhilling, on a piece of tin-foil, twelve inches in length and three in breadth.

Then the foil is to be inſtantly rolled up, ſo as to include the ſalt, as it lay, between the coils. The ends are to be ſhut by pinching them toge-

ther, and the whole is to be preſſed flat and cloſe.

All this being done as quickly as poſſible, the firſt phænomenon is—A part of the ſalt deliqueſces.

2d. This part, impregnated with tin, changed in colour, and of a thicker conſiſtence, begins to froth forth from the ends of the coil.

3d. A ſtrong frothing, accompanied with moderate warmth.

4th. The emiſſion of copious nitrous fumes.

5th. Heat intolerable to the fingers.

6th. Exploſion and fire, which burſt and fuſe the tin-foil in ſeveral places, if it be very thin.

After many conjectures and experiments, I diſcovered a property in the cupreous ſalt, from which, and the known affinities of the bodies concerned, theſe appearances, however new and ſingular, may be accounted for.

The cupreous ſalt properly dried and placed where it may receive a heat, not much greater than what the hand can bear, takes fire. The circumſtances which favour this ignition, and contribute to produce it in the ſmalleſt degree of heat, concur in the following convenient method of trying the experiment.

A piece of ſoft bibulous paper is to be dipped in the nitrous ſolution of copper, and dried before the fire two or three times alternately. Then it is to be approached towards the heat, as near as can be borne, by the hand which holds it without pain: there, if it has been ſufficiently dried, it will preſently catch fire, and burn to a brown calx.

The

The eaſy ignition of the ſalt in a ſlight heat being thus aſcertained, there is no room to doubt that the foregoing phænomena are produced in the following manner.

The acid of the liquor, which moiſtened the ſalt, quits the copper to unite with the tin, leaving the water to be imbibed by the contiguous ſalt of copper, which then diſſolves, and acts briſkly on the tin-foil.

It is well known that the action of the nitrous acid on tin is always accompanied with conſiderable heat and efferveſcence, and that the ſolution of metallic ſalts in watry liquors is haſtened by heat.

In our experiment, the warmth generated by the firſt action of the cupreous ſolution, promotes the deliqueſcence of the cryſtallized ſalt. The union of the acid with the tin is rapid, not only as being aſſiſted by heat, but on account of the great ſurface expoſed; whence the ſtrong frothing, and the extraordinary heat, by which the redundant moiſture is carried away, and the undecompoſed part of the cupreous ſalt, together with that lately formed with the tin, perfectly dried.

The heat generated upon both ſurfaces of a large expanſe of tin, is concentrated by cloſely coiling it into a ſmall compaſs, and being retained by the various ſurrounding laminæ of metal, it is neceſſarily accumulated to a quantity, which, if we may judge from the touch, is more than ſufficient to fire the dry cupreous ſalt.

The ſalt formed with tin, and the nitrous acid, burns and ſparkles in a red heat. Catching fire, therefore, from the ignited cupreous ſalt, it burns

with it, and aſſiſts in the detonation, which is common to all nitrous compoſitions in ſimilar circumſtances.

If the ſalt be very wet, there will not be much fire or exploſion, becauſe the heat will be diſſipated before the ſalt can be ſufficiently dried in every part.

If the ſalt be not moiſt, it cannot commence the action which is neceſſary; and there will be no fire, becauſe there can be no haſty ſolution of the tin to give the requiſite heat.

If the tin and ſalt be not coiled up in due time, there will be very little heat and no fire; becauſe the diſſipation of the heat from a broad expanſe, keeps pace with the generation of it; and as the moiſture exhales quickly in this manner, there is none left to renew the action on the tin and conſequent heat, when the proper time of coiling has elapſed.

A piece of tin-foil, larger than that I have deſcribed, cannot eaſily be managed; ſmaller pieces give leſs fire in the direct proportion of their ſurfaces, and the quantity of ſalt which they can, at the ſame inſtant, reduce to the required ſtate of dryneſs.

The ſudden diſſipation of the moiſture appears to me the moſt curious of theſe phænomena. To render it the more obſervable, I made the following experiments.

I placed a piece of tin-foil, twelve inches long by two broad, looſely coiled, and ſtanding vertically on the flatteſt end, in half a table-ſpoonful of the ſaturated ſolution of copper in the diluted nitrous acid, and found that ſcarce five ſeconds elapſed

elapſed from the time, when a briſk effervescence, accompanied with weak nitrous fumes, aroſe, until the liquor became a conſiſtent maſs, and ſparks of fire iſſued from the coils of tin; which having attracted part of the ſolution above the common level, brought it into the condition in which it is readily dried, heated, and fired.

A like quantity of the ſame ſolution, kept in a ſtrong boiling heat, does not acquire ſuch conſiſtence in a ten-fold ſpace of time.

The haſty exhalation, therefore, is not cauſed by the heat alone; neither does it ſeem to require any great ſurface. What elſe it is owing to, I commit for a while to the examination of the curious.

Orchard-Street, Portman-Square, Dec. 23, 1772.

B. Higgins.

Received

Received Nov. 12, 1772.

XVII. *Extracts of ſome Letters, from Sir* William Johnſon *Bart. to* Arthur Lee, *M. D. F. R. S. on the Cuſtoms, Manners, and Language of the* Northern Indians *of* America.

Read Jan. 28, 1773.

IN all enquiries of this ſort, we ſhould diſtinguiſh between the more remote tribes, and thoſe Indians, who, from their having been next to our ſettlements for ſeveral years, and relying ſolely on oral tradition for the ſupport of their antient uſages, have loſt great part of them, and have blended ſome with our cuſtoms, ſo as to render it extremely difficult, if not impoſſible, to trace their cuſtoms to their origin.

The Indians did certainly live under more order and government formerly, than at preſent. This may ſeem odd, but it is true; for, their intercourſe being with the lower claſs of our traders, they learn little from us but our vices; and their long wars, together with the immoderate uſe of ſpirituous liquors, have ſo reduced them, as to render that order, which was firſt inſtituted among them, unneceſſary and impracticable.

They

They do not at preſent uſe hieroglyphics; their figures being drawn, to the utmoſt of their ſkill, to repreſent the thing intended. For inſtance, when they go to war, they paint ſome trees with the figures of warriors, often the exact number of the party; and if they go by water, they delineate a canoe. When they gain a victory, they mark the handle of their tomahawks with human figures, to ſignify priſoners; and draw the bodies without heads, to expreſs the ſcalps they have taken. The figures which they affix to deeds have led ſome to imagine, that they had alphabetical characters or cyphers. The fact is this. Every nation is divided into tribes, of which ſome have three, as the turtle, bear, and wolf; to which ſome add the ſnake, deer, &c. Each tribe forms a little community within the nation; and as the nation has its peculiar ſymbol, ſo has each tribe the particular badge from which it is denominated: and a Sachem of each tribe being a neceſſary party to a fair conveyance, ſuch Sachem affixes the mark of his tribe thereto, like the public ſeal of a corporation. With reſpect to the deed of 1726, of which you ſent me the ſignatures, the tranſaction was in ſome meaſure of a partial nature. All the nations of the confederacy did not ſubſcribe it; and thoſe chiefs who did, neglected to pay due regard to their proper ſymbols; but ſigned agreeably to fancy, of which I have ſeen other inſtances. The manner I have mentioned is the moſt authentic, and conformable to their original practice.

As to the information, which, you obſerve, I formerly tranſmitted to the Governor of New York,

York, concerning the belt and 15 bloody ſticks ſent by the Miſſiſagees, the like is very common; and they uſe theſe ſticks, as well to expreſs the alliance of caſtles, as the number of individuals in a party. The ſticks are generally about 6 inches in length, very ſlender, and painted red if the ſubject be war. Their belts are moſtly black wampum, painted red when they denote war. They deſcribe caſtles ſometimes upon them, by ſquare figures of white wampum; and in alliances, human figures holding a chain, which is their emblem of friendſhip, and each figure repreſents a nation. An axe is alſo ſometimes deſcribed, and always imports war: the taking it up, being a declaration of war; and the burying it, a token of peace.

With reſpect to your queſtions concerning the chief magiſtrate, or Sachem, and how he acquires his authority, &c.; I am to acquaint you, that there is, in every nation, a Sachem, or chief; who appears to have ſome authority over the reſt, and it is greateſt amongſt the moſt diſtant nations. But in moſt of thoſe bordering on our ſettlements, his authority is ſcarcely diſcernible, he ſeldom aſſuming any power before his people. And indeed this humility is judged the beſt policy; for, wanting coercive power, their commands would perhaps occaſion aſſaſſination, which ſometimes happens.

The Sachems of each tribe are uſually choſen in a public aſſembly of the chiefs and warriors, whenever a vacancy happens by death or otherwiſe; they are generally choſen for their ſenſe

and bravery from among the oldeſt warriors, and approved of by all the tribe; on which they are ſaluted Sachems. There are, however, ſeveral exceptions; for ſome families have a kind of inheritance in the office, and are called to this ſtation in their infancy.

The chief Sachem, by ſome called the King, is ſo either by inheritance or by a kind of tacit conſent, the conſequence of his ſuperior abilities and influence. The duration of his authority depends much on his own wiſdom, the number and conſequence of his relations, and the ſtrength of his particular tribe. But even in thoſe caſes where it deſcends, ſhould the ſucceſſor appear unequal to the taſk, ſome other Sachem is ſure to poſſeſs himſelf of the power and the duties of the office. I ſhould have obſerved, that military ſervices are the chief recommendations to this rank. And it appears pretty clearly, that heretofore the chief of a nation had, in ſome ſmall degree, the authority of a ſovereign. This is now the fact among the moſt remote Indians. But as, ſince the introduction of fire arms, they no longer fight in cloſe bodies, but every man is his own General; I am inclined to think this has contributed to leſſen the power of a chief. This chief of a whole nation has the cuſtody of the belts of wampum, &c. which are as records of public tranſactions: he prompts the ſpeakers at all treaties, and propoſes affairs of conſequence. The chief Sachems form the grand council; and thoſe of each tribe often deliberate apart on the affairs of their particular tribes. All their deliberations

are conducted with extraordinary regularity and decorum. They never interrupt him who is ſpeaking; nor uſe harſh language, whatever may be their thoughts.

The chiefs aſſume moſt authority in the field: but this muſt be done, even there, with great caution; as a head warrior thinks himſelf of moſt conſequence in that place.

The Indians believe in, and are much afraid of, witchcraft: thoſe ſuſpected of it are therefore often puniſhed with death. Several nations are equally ſevere on thoſe guilty of theft, (a crime indeed uncommon among them): but in caſes of murder, the relations are left to take what revenge they pleaſe. In general, they are unwilling to inflict capital puniſhments, as theſe defeat their grand political object, which is, to increaſe their numbers by all poſſible means.

On their hunts, as upon all other occaſions, they are ſtrict obſervers of *meum* and *tuum*, and this from principle, holding theft in contempt; ſo that they are rarely guilty of it, though tempted by articles of much value. Neither do the ſtrong attempt to ſeize the prey of the weak; and I muſt do them the juſtice to ſay, that, unleſs heated by liquor or inflamed by revenge, their ideas of right and wrong, and their practices in conſequence of them, would, if more known, do them much honor. It is true, that, having been often deceived by us in the purchaſe of lands, in trade, and other tranſactions, many of them begin now to act the ſame part. But this reflects moſt on thoſe who ſet them the example.

As

As to your remark on their apparent repugnance to civilization, I muſt obſerve, that this is not owing to any viciouſneſs of their nature, or want of capacity; as they have a ſtrong genius for arts, and uncommon patience. I believe they are put to the Engliſh ſchools too late, and ſent back too ſoon to their people, whoſe political maxim, Spartan-like, is to diſcountenance all purſuits but war, holding all other knowledge as unworthy the dignity of man, and tending to enervate and divert them from that warfare on which they conceive their liberty and happineſs depend. Theſe ſentiments conſtantly inſtilled into the minds of youth, and illuſtrated by examples drawn from the contemptible ſtate of the domeſticated tribes, leave laſting impreſſions; and can hardly be defeated by an ordinary ſchool education.

I wiſh my preſent leiſure would allow me to give you as many ſpecimens of their language as would ſhew, that (though not very wordy) it is extremely emphatical; and their ſtyle adorned with noble images, ſtrong metaphors, and equal in allegory to many of the Eaſtern nations. The article is contained in the noun, by varying the termination; and the adjective is combined into one word. Thus of *Echin*, a man, and *Gowana*, great, is formed *Echingowana*, a great man. *Cahyunghaw* is a creek, *Caghyungha* a river, *Caghyunghaowana* a great river; *Caghyungheeo* a fine river. *Haga* the inhabitants of any place, and *Tierham* the morning; ſo, if they ſpeak of Eaſtern people, they ſay *Tierhanſ-aga*, or people of the morning. *Eſo* is expreſſive of a great quantity,

and *Eſogee* is the ſuperlative. The words *Goronta* and *Golota*, which you mention, are not of the ſix nations, but a Southern language. It is curious to obſerve, that they have various modes of ſpeech and phraſes peculiar to each age and ſex, which they ſtrictly obſerve. For inſtance, a man ſays, when he is hungry, *Cadagcariax*, which is expreſſive both of his want and of the animal food he requires to ſupply it; whilſt a child ſays, in the ſame circumſtances, *Cautſore*, that is, I require ſpoon-meat.

There is ſo remarkable a difference in the language of the ſix nations from all others, as affords ground for enquiring into their diſtinct origin. The nations North of the St. Lawrence, thoſe Weſt of the great lakes with the few who inhabit the ſea-coaſts of New England, and thoſe again who live about the Ohio, notwithſtanding the reſpective diſtances between them, ſpeak a language radically the ſame, and can in general communicate their wants to one another; while the ſix nations, who live in the midſt of them, are incapable of conveying a ſingle idea to their neighbours, nor can they pronounce a word of their language with correctneſs. The letters *M* and *P*, which occur frequently in the other languages, are not in theirs; nor can they pronounce them but with the utmoſt difficulty. There is indeed ſome difference of dialect among the ſix nations themſelves; but this is little more than what is found in all the European ſtates.

Received

Received January 15, 1772.

XVIII. *An Account of ſome curious Fiſhes, ſent from* Hudſon's Bay; *by Mr.* John Reinhold Forſter, *F. R. S. in a Letter to* Thomas Pennant, *Eſq*; *F. R. S.*

DEAR SIR,

Read Jan. 28, 1773. THE Governor and Committee of the Hudſon's Bay Company preſented the Royal Society with a choice collection of ſkins of quadrupeds, many fine birds, and ſome fiſh, collected by their ſervants at the ſeveral ports in Hudſon's Bay; the Committee of the Royal Society, for examining and deſcribing theſe curioſities, did me the honour to refer them to me for examination. I wiſh the following obſervations on the fiſh, which I take the liberty to addreſs to you, as a lover of Natural Hiſtory, and my remarks on the birds and quadrupeds, may convey ſuch informations concerning the zoology of North America, in the ſtudy of which you have made ſuch vaſt progreſs; and ſo particularize the animals of the *Arctic* part of that vaſt continent, that nothing further may prevent

your favouring the public with the reſult of your ſtudies on that ſubject.

The four kinds of Hudſon's Bay fiſh are the *Sturgeon*, the *Burbot*, the *Gwiniad*, and a new fiſh called the *Sucker* at Hudſon's Bay.

The Sturgeon was about fourteen inches long, and therefore ſeems to be a young fiſh; as it is likewiſe obſerved in the liſt, written by the Gentleman who ſent this fiſh from York Fort.

Description.

Its noſe is very long and ſlender, terminating in a point; the eyes are ſmall; under the projecting ſnout, before the mouth, are four beards or *cirri*, placed nearly in the ſame line, and not by pairs, as in ſome other ſpecies of Sturgeon. The mouth is beneath, nearly oppoſite the eyes, toothleſs, cartilagineous, ſemilunar when in its natural poſition, but round when open; on each ſide are two noſtrils. The whole head is depreſſed, and very nearly quadrangular; the whole body pentagonal, and tapering towards the tail; the whole ſkin tough, covered with five rows of uncinated ſcales; the dorſal ſeries conſiſts of fourteen large roundiſh ſcales, and a ſingle one behind the dorſal fin; each of the lateral rows has 35 oblique ſcales; in the two ventral rows are nine roundiſh ſtrong ſcales between the pectoral and ventral fins; one ſcale is behind the vent, and ſtill another behind the anal fin.

The fiſh, according to this deſcription, ſeems to come the neareſt to that ſpecies of Sturgeon which I de-

I deſcribed in the Philoſ. Tranſactions, Vol. LVII. in my *Specimen Hiſtoriæ Naturalis Volgenſis*, N° 10. under the name of *Acipenſer Ruthenus major, roſtro elongato acuminato, paululum ſupino*, and which the Ruſſians call *Sevruga*. Kramer, in his *Elenchus Vegetabilium & Animalium Auſtriæ*, p. 383. is the only writer that I know who takes notice of this ſpecies; he calls it *Acipenſer roſtro acuto, corpore tuberculis ſpinoſis aſpero:* the inhabitants of Auſtria call it *Shirk*, a name they have no doubt borrowed from the Slavonian name *Sevruga*. The famous painter and traveller Cornelys de Bruyn mentions this kind of fiſh, but in ſo ſuperficial a manner, that one plainly ſees he was little, if at all, uſed to diſcuſſions in points of Natural Hiſtory. He ſays, * "the *Sterlet* is the "beſt fiſh in Ruſſia; there are two ſpecies of it; "but, upon the whole, it is nearly related to the "Sturgeon. The *Severukas* differ in nothing from "the Sturgeon, which the Ruſſians call *Aſſetrina*. "The Caviar is made from the *Beluga*, the *Aſ-* "*ſetrina*, and the *Severuka*." Had de Bruyn examined the *Sevruga*, he would certainly have found it materially different from the *Oſetr* or *Aſſetrina*, i. e. the common blunt-noſed Sturgeon of Germany and the Baltick. I ſuppoſe the Engliſh Sturgeon, from your own deſcription †, and the drawing in the Britiſh Zoology, illuſtrated by plates, tab. LXXXIX. to be the ſame with this kind from Hudſon's Bay, and with the *Sevruga* of the Ruſſians, and the *Shirk* of the Auſtrians. The true Sturgeon, which gave

* De Bruyn's Voyage, Tom. I. &c. Amſterd. fol. p. 93.
† Br. Zool. octavo, Vol. III. p. 99.

the

the name to the whole genus *, I think to be an unknown fiſh in England. The ſpecies of Sturgeons are more numerous than one is at firſt aware of; and it would therefore be of ſome utility, that perſons, who have an opportunity of examining all the various kinds at Vienna, and in Ruſſia, might do it with more care than has hitherto been done. Some of the ſorts which I have ſeen, I have ſo deſcribed that they may be known again; ſome I did not ſee, and gave their characters from books, and from the reports of ſuch perſons as had examined them. Mr. Klein, a very ingenious naturaliſt, has enumerated ten Sturgeons, in his 4th *Miſſus Piſcium*, p. 11—16. and Count Marſigli, in his ſplendid work about the Danube, Tom. IV. gives the names of at leaſt ſix Sturgeons, but the characters are not ſufficiently ſettled in both theſe works. Klein ſaw but two kinds of Sturgeons, and a third in ſpirits; and Count Marſigli was not enough of a naturaliſt to give adequate deſcriptions of theſe fiſh. Therefore it is certain that a careful examination and accurate account of the ſeveral ſpecies of Sturgeons would greatly illuſtrate the Natural Hiſtory of this genus.

The ſecond of the Hudſon's Bay fiſh, is called, by the wild natives of that country, *Marthy*, and is nothing elſe than our common Burbot, *Gadus Lota*, Linn. only vaſtly ſuperior in ſize. The deſcriptions

* The Germans call this fiſh *Stoer*, from the old Teutonic word *Stor* or *Stuhr*, which ſignifies *great*, as this fiſh grows to a very large ſize. Thus likewiſe the Scotch call the *Tunny*, Mackrel *Sture*. Vide Mr. Pennant's Tour in Scotland, p. 192.

you

you have given of this fiſh, in the Britiſh Zoology, is entirely correſponding with this ſpecimen, ſo that it would be ſuperfluous to preſume to make any additions to it. I muſt, however, obſerve, that, after a moſt minute examination, I could find no more than ſix branchioſtegous rays in the two ſpecimens from Hudſon's Bay, of which you mention ſeven in the Engliſh Burbot, and Artedi as many in his ſpecimens. This great naturaliſt ſeems likewiſe to be right, when he obſerves that the *cirri*, or beards on the end of the noſe, are the valves to one of the noſtrils; for I found that theſe beards, on their under-ſide, opened into a hole, correſponding with the lower noſtril. Mr. Andrew Graham, the collector of the Natural Hiſtory ſpecimens at Severn River in Hudſon's Bay, obſerves, that theſe fiſh conſtantly ſwim cloſe to the ground, and are extremely voracious; for he repreſents them as not content with devouring every fiſh * they can overcome, but likewiſe feeding on putrefying deer, or other carrion that comes in their way; even ſtones are ſometimes ſwallowed to ſatisfy their inſatiable appetite, of which Mr. Graham was himſelf a witneſs, having taken a ſtone of a pound weight out of the ſtomach of this fiſh. The pike is often obliged to fall a victim, together with the trout, *Tickomeg*, and others, to this rapacious fiſh. After ſunſet, it is caught by a night-hook. It does not maſticate its food before deglutition. Its roe and liver are reckoned a delicacy, when freſh caught; but they turn rancid and

* This too is the fiſh that makes ſuch havock in the Lake of Geneva. P.

oily in a few days, though kept frozen ſolid all the time. At Hudſon's Bay this fiſh is thought to be dry and inſipid; its weight is from one to eight pounds.

The third ſpecies of fiſh, from this cold climate, is by the natives called *Tickomeg*, and is our *Gwiniad* or *Salmo Lavaretus*, Linn.; only the ſize is ſomewhat bigger, for the greateſt ſpecimen ſent over meaſures 18 inches from the head to the tip of the tail, is $4\frac{1}{5}$ inches deep, and not above an inch and $\frac{1}{4}$ thick. This fiſh differs in no circumſtance from our Gwiniad, but the length. You mentioned in your Britiſh Zoology (Vol. III. p. 269.) a *Ferra* or *Gwiniad* from Switzerland 15 inches long, as an uncommon ſize *; the Hudſon's Bay fiſh, as I have before obſerved, is 18 inches long, and $4\frac{1}{5}$ inches its greateſt depth. The great abundance of food, and the ſmall number of inhabitants, who let the fiſh grow up undiſturbed, are perhaps the cauſes of their uncommon bigneſs. They weigh from $1\frac{1}{2}$ pound to 3 pounds, ſays Mr. Graham; but, I am ſure, the fiſh I examined muſt, when freſh, have weighed more. Theſe fiſh abound in the River Severn in Hudſon's Bay, from its origin in the great lakes to its mouth, where it empties itſelf into the bay. The natives catch five or ſix hundred a day, by means of wears which they contrive in the river: they will not take bait, and are poor at the breaking of the ice in the river. In the middle of the ſummer, after a gale of wind,

* However, the Gwiniads of Lapland, a ſimilar climate to that of the Hudſon's Bay, are vaſtly large. Brit. Zool. III. 267. note.

they are often found thrown up into the marſhes, and on the ſhoals, where they remain at the receſs of the water and abating of the wind, and ſerve as food to numbers of crows. The inhabitants of Hudſon's Bay think this fiſh very ſweet, and good to eat, contrary to the opinion of many Europeans.

The fourth and laſt fiſh brought from Hudſon's Bay is there called a *Sucker*, becauſe it lives by ſuction, according to Mr. Graham's account, who likewiſe ſays, that there are two varieties of this fiſh, both of a whitiſh colour, but one diſtinguiſhed by a mixture of beautiful red. In the ſmalleſt of two ſpecimens brought over, a broad ſtripe of red could be obſerved all along the *linea lateralis*. They are very numerous in the creeks and rivers, and troubleſome in overburdening the nets. They are not deemed a palatable food, being very ſoft, and full of ſmall bones. They weigh from one half to 2½ pounds.

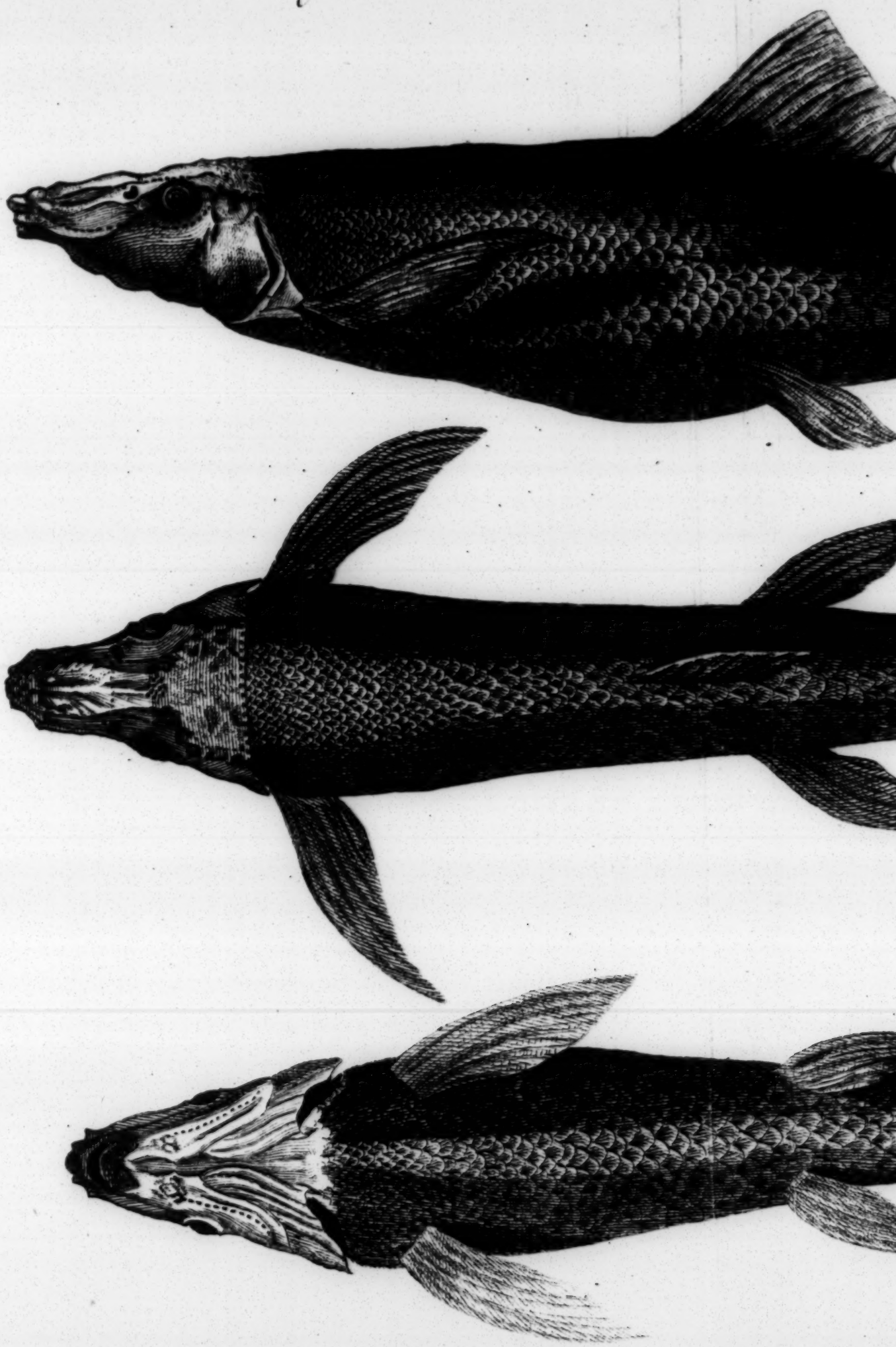
The above is literally what Mr. Graham ſays of this fiſh, and all that is known of its natural hiſtory. Examining it carefully, I found it was a new ſpecies of the genus of *Cyprinus*, or *Carp*.

The head is broader than the body, gradually decreaſing towards the noſe, full of elevations and tubercles, nearly quadrangular, and not ſcaly. The mouth is quite under the head, as in the *Loricariæ*, when ſhut, ſemilunar; when open, round; not far from the extremity of the ſnout, and included in ſmall round lips. To the under-lip is fixed a bilobated, beard-like, papilloſe caruncula; it has no teeth. The

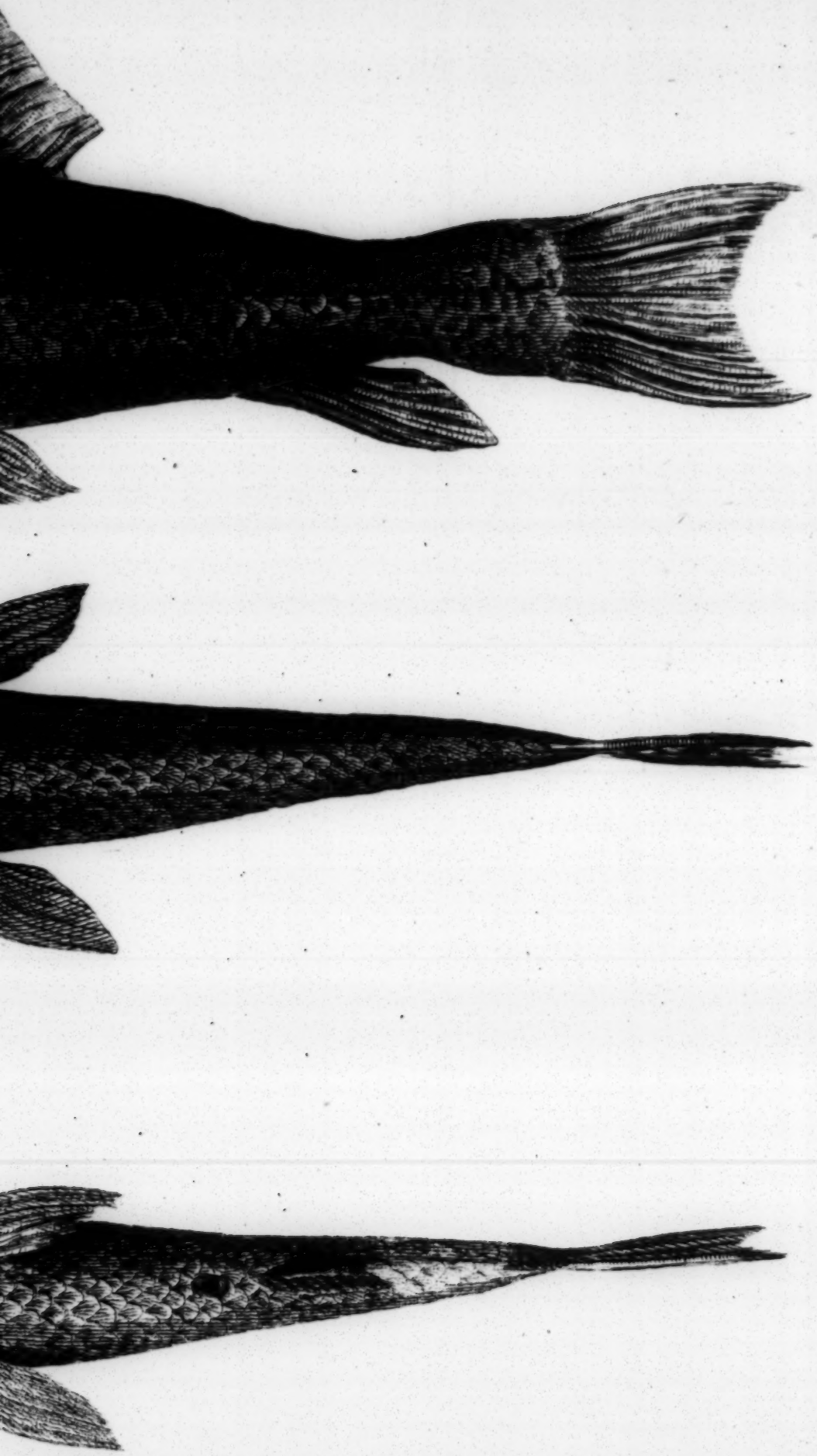
eyes are large, but the colour of the iris could not be determined. The number of the branchioſtegous rays is three. The body is flat, tapering towards the tail, and ſcaly. The greater ſpecimen meaſures very near 15 inches from the noſe to the extremity of the tail; next to the head it is nearly two inches thick, about the dorſal fin $1\frac{1}{4}$ inch; its greateſt depth before the ventral fins is $2\frac{4}{5}$ inches. On the ſnout are about five round prominent tubercles; two noſtrils are found on each ſide, the biggeſt next before the eye is kidney-ſhaped. The covers of the gills are double, and divided; the head has ſeveral ſutures; over each eye, in a cavity, are two longitudinal ones, joined oppoſite the noſtrils by a ſtill ſhorter tranſverſe one; on the covers of the gills are two, on each ſide one, beginning near the lobes of the caruncula of the under-lip, and going up arched towards the eye. Near the extremity of the ſnout begins on each ſide a longitudinal one; it paſſes under the eye, and mounts in a curvature behind it, then it goes on ſtraight to the end of the head, where it again gets downwards, and joins the lateral line. Where the head joins to the body, theſe two ſutures are connected by a tranſverſal one, which, as it were, ſeparates the head from the body. The lateral line at firſt deſcends from the head, but then runs on ſtraight, rather nearer the back than the body, to the beginning of the tail. The ſcales are ſmall near the head and back, increaſing in ſize towards the middle and tail, cloſe to which they are again ſmaller. The dorſal fin is placed ſomewhat behind the equilibrium of the fiſh, rhomboidal, and conſiſting of twelve ſtrong branched rays. The pectoral fins are

lanceo-

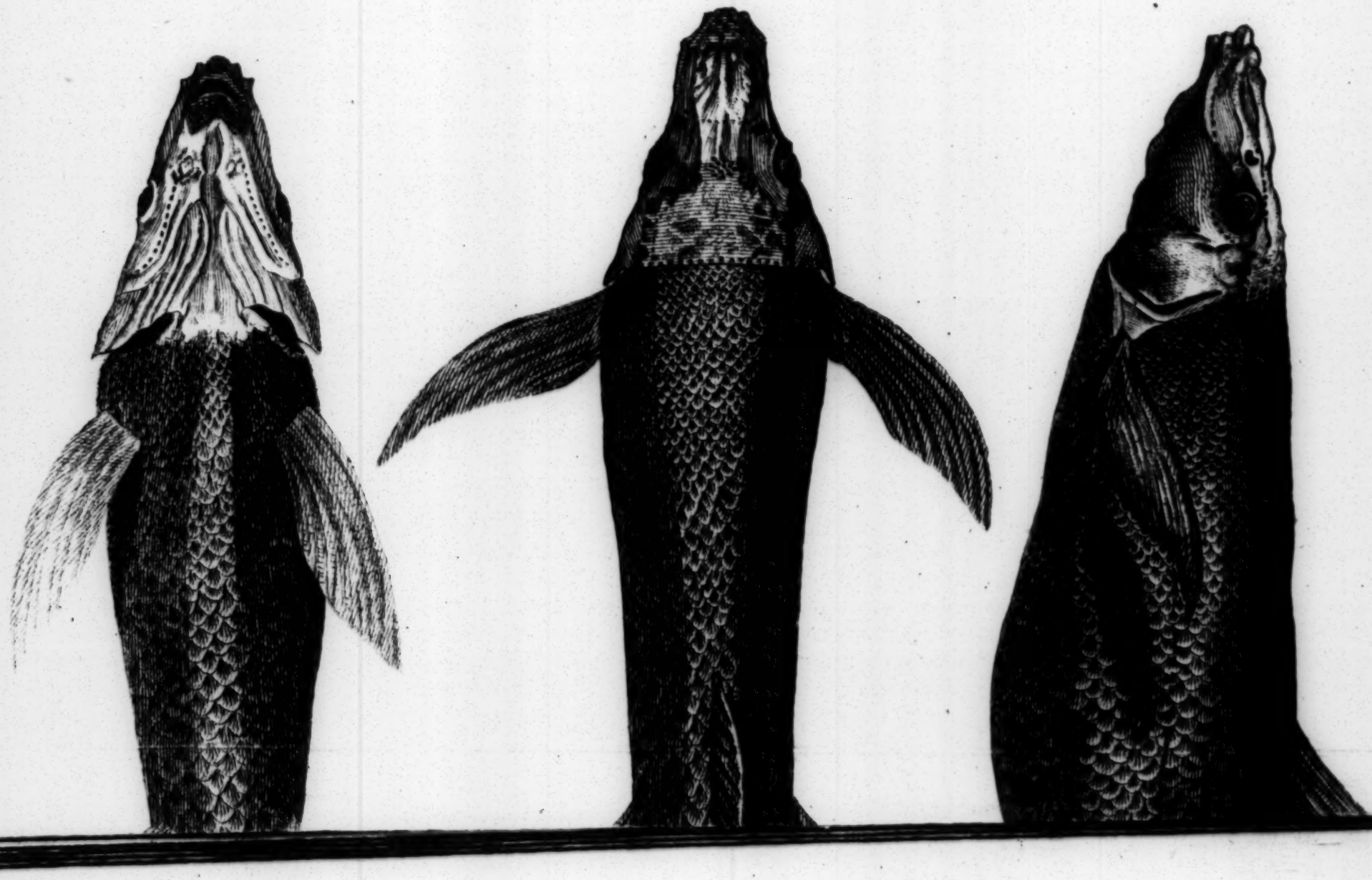
Cyprinus Catostomus.



The Sucker.



A. Basire sculp.



Cyprinus Catostomus.

lanceolated, fixed under the covers of the gills, and have 17 rays. The ventral fins have 10 or 11 rays, and are placed in the middle of the belly, and under the dorſal fin. The anal fin conſiſts of eight branched ſtrong rays. The tail is ſomewhat forked or concave, and conſiſts of ſeventeen rays.

I have been as circumſtantial as poſſible in deſcribing this new ſpecies, and join here, together with a drawing [See TAB. VI.], a Latin ſcientific deſcription of the ſame.

I am,

With the trueſt regard,

DEAR SIR,

Your moſt obedient,

humble ſervants,

Jn° Reinhold Forſter.

N° 2. Somerſet Stable-yard, Strand,
January 12, 1772.

Κατω

Κατω
infra
Στομαος. } CYPRINUS Catoſtomus.

CYPRINUS pinna ani radiis VIII. labio imo caruncula bilobata papilloſa, cauda bifida. Pinnæ D. 12. P. 17. V. 10.—11. A. 8. C. 17.
Habitat in Sinus Hudſonis fluminibus copioſe, ſugendo paſcitur. Anglis, the *Sucker*.

DESCR. *Caput* ſubtetragonum, verſus apicem ſenſim attenuatum, obtuſiuſculum, corpore fere craſſius, & minus latum.
Tubercula globoſa, confertiora in apice roſtri, circiter quinque; carinata & acuminata, in vertice ſparſa.
Foramina (ſive nares) gemina, quorum alterum minus, alterum oculis proximum, majus, reniforme.
Oculi magni, ad marginem ſuperficiei verticalis capitis ſiti, fere in medio inter apicem & baſin. *Irides* . . . . .
Opercula branchiarum magna, nuda; at ſub oculis opercula ſpuria, primo intuitu pro radiis membranæ branchioſtegæ facile ſumenda.
Suturæ in capite plures catenulatæ; una utrinque brevis, ſupra oculos, nareſque, nec baſin nec apicem capitis attingens, è regione narium juncta per ſuturam tranſverſalem breviſſimam; ſecunda utrinque incipiens ad angulum loborum caruncularæ,

carunculæ, imo labio adnata, in operculo ſpurio recurvatur, & prope oculos definit: tertia utrinque incipiens prope roſtri apicem, linea recta ſub oculis ducta, dein curvatur & aſcendit verſus verticem; ibi rurſus curvatur & jungitur lineæ laterali, pone aperturam branchialem deſcendenti: connectuntur hæ duæ lineæ laterales ſutura tranſverſa, quæ caput à reliquo corpore diſtinguit.

Membrana Branchioſtega, radiis tribus brevibus, validis.

Rictus inferus, lunulatus, ſeu ſemiorbicularis, labiis incluſus tenuibus, ſuperiore (ore ſcilicet clauſo) concavo, inferiore convexo.

Caruncula lata, labio inferiori adnata, craſſiuſcula, carnoſa, papillis tecta, oris angulos ambiens, medio in lobos binos profunde diviſa.

CORPUS lateribus compreſſiuſculum, at verſus abdomen magis compreſſum, cuneiforme, capite ad caudam ſenſim attenuatum, tectum ſquamis minoribus, ovatis, ſtriatis verſus caput minimis, pallide argenteis, in quibuſdam circa lineam lateralem aureo-rubris.

Linea lateralis recta, dorſo parallela, ad caput ſuper aperturam branchialem adſcendens.

Anus

Anus parvus, caudæ multo proprior quam capiti.

Pinna dorſi pone æquilibrium nonnihil poſita, rhomboidalis, radiis validis, ramoſis duodecim.

Pinnæ pectorales lanceolatæ, infra opercula ſitæ radiis 17 longitudine partem quartam totius piſcis (excluſis capite & caudâ) æquant.

Pinnæ ventrales radiis 10 vel 11 oblongæ, in medio ventris, ſub pinna dorſali poſitæ, pinna pectorali dimidio breviores.

Pinna ani caudæ propinqua, longitudine fere pinnæ pectoralis, radiis octo validis, ramoſis.

Cauda leviter bifurca, pinnam pectoralem longitudine & numero radiorum æquans.

Longitudo totius piſcis unciarum 15 pedis Anglicani.

Latitudo unciarum circiter 3 ante pinnas ventrales.

Craſſities corporis prope caput unciarum fere 2, ante pinnam dorſalem unciæ & quadrantis.

P. S. Beſides the above-mentioned fiſh, the ſervants of the Hudſon's Bay Company have likewiſe ſent over from thence the common *River Crayfiſh* (Cancer Aſtacus, *Linn.*), which, in every particular, correſponds with the Engliſh one.

Received

William Withering, *M. D.* *Communicated*
S.

; ſeparation, was itrous acid ; but	One dram of each of the marles being calcined, weighed			The calcined marles put into water, produced
/hen burnt	Grains.	Loſt grains.	Burnt to	
red brick.	52	8	Red brick.	No effect.
red brick.	53	7	Red brick.	No effect.
ellowiſh brick.	49	11	Soft yellow brick.	Weak lime water.
red brick.	50	10	Red brick.	No effect.
ale red brick.	48	12	Hard grey ſtone.	Lime water.
on.	51	9	Soft and ſtony.	Lime water.
coheſion.	46	14	Soft ſtone.	Lime water.
ed brick.	48	12	Soft ſtone.	Strong lime water.
on.	43	17	Soft ſtone.	Strong lime water.
on.	48	12	Soft ſtone.	Strong lime water.
on.	40	20	Soft ſtone.	Strong lime water.
whitiſh brick.	29	31	Powdery.	Strong lime water.

ſced; N° 1 and 2 the leaſt, N° 12 the moſt. The effervescence having
r to a red colour. To the filtered colours was gradually added mild fixed
/ater, till free from all ſaline matter, weighed, when dry, as in column the
N° 12 a white clay; in N° 8 a red clay, and a portion of ſand; in N° 3 a
ſiduums were all waſhed with rain water before they were burnt. The pre-
ght. Rain water was poured upon the calx; the next morning there was a
ly. The marles were kept for ſome weeks in a dry place before they were
of Bridgewater's canal, in a powdery form, and when mixed with one fourth

es of this country into quicklime, for the purpoſes of agriculture; they may
s the calcarious earth united with clay, as in N° 1, 2, 4, &c. may be the
already ſtiff, and abounding with clay. How far the different quantities of
agriculture, muſt be left to the experience of the farmer to determine.

Received

Received Nov

XIX. *Experiments upon the different Kinds of Marle found i*

by Charles Morton,

Read Feb

Number.	Deſcription.	Quantity of calcarious earth in half a dram, as ſeparated by the nitrous acid, and precipitated by mild fixed alkaly.	Wh n b
		Grains.	Mix
1.	Red and blue intermixed, in ſmall friable lumps.	1	Uni
2.	Red, in ſmall friable lumps.	$0\frac{3}{4}$	Uni
3.	Grey, in large hard lumps.	5	Pla
4.	Red, hard, compact.	3	Uni
5.	Red, with grey ſpots, in large hard lumps, ſcarcely to be broken with a hammer.	$8\frac{1}{2}$	Pla
6.	Light grey, like a grit ſtone.	8	Gri
7.	Brown, friable, in large lumps.	18	No
8.	Red, in large friable lumps.	14	Pla
9.	Browniſh white, very hard, like calcarious incruſtations.	16	No
10.	Lead colour, friable, flaky.	$14\frac{1}{2}$	No
11.	Brown grey, very hard, in irregular lumps.	16	No
12.	Lead colour, in powder and in ſmall hard lumps.	$20\frac{1}{2}$	Uni

Half a dram of the marles being put into ſimilar glaſs cups, two drams of nitrous acid being added ceaſed, and ſix drams of rain water being added to each glaſs, the liquors were all filtered, and after alkali, ſufficient to ſaturate the acid, and precipitate all the earth it had diſſolved. The precipitated third. Column the fourth ſhews that, after the ſeparation of the calcarious earth, there remained in whitiſh clay, with a portion of ſand; in N° 6, 9, 10, 11. pure ſand; and in N° 7. ſand, with a ſm cipitated powders being mixed together, 82 grains thereof put into a crucible, and calcined with a ſtro pellicle upon the ſurface of the water; it taſted ſtrongly of lime, and let fall a calcarious earth, upon t made uſe of. They were all got out of marle pits in the neighbourhood of Stafford, except N° 12, w part of clay is burnt to quick lime. All the above marles crack and fall to pieces, when expoſed to t

The foregoing experiments were undertaken with a view to aſcertain how far it would be adviſeabl likewiſe furniſh us with ſome uſeful hints relative to the kind of marles proper to be uſed upon diffe beſt for light ſandy ſoil; and N° 6, 9, 10, 11, where the calcarious earth is united with ſand, the m fixable air, or other volatile parts, contained in each of the marles, as ſhewn by column the fifth, wi

November 12, 1772.

d in Staffordſhire, *by* William Withering, *M. D.* *Communicated* ton, *M. D. Sec. R. S.*

d Feb. 4, 1773.

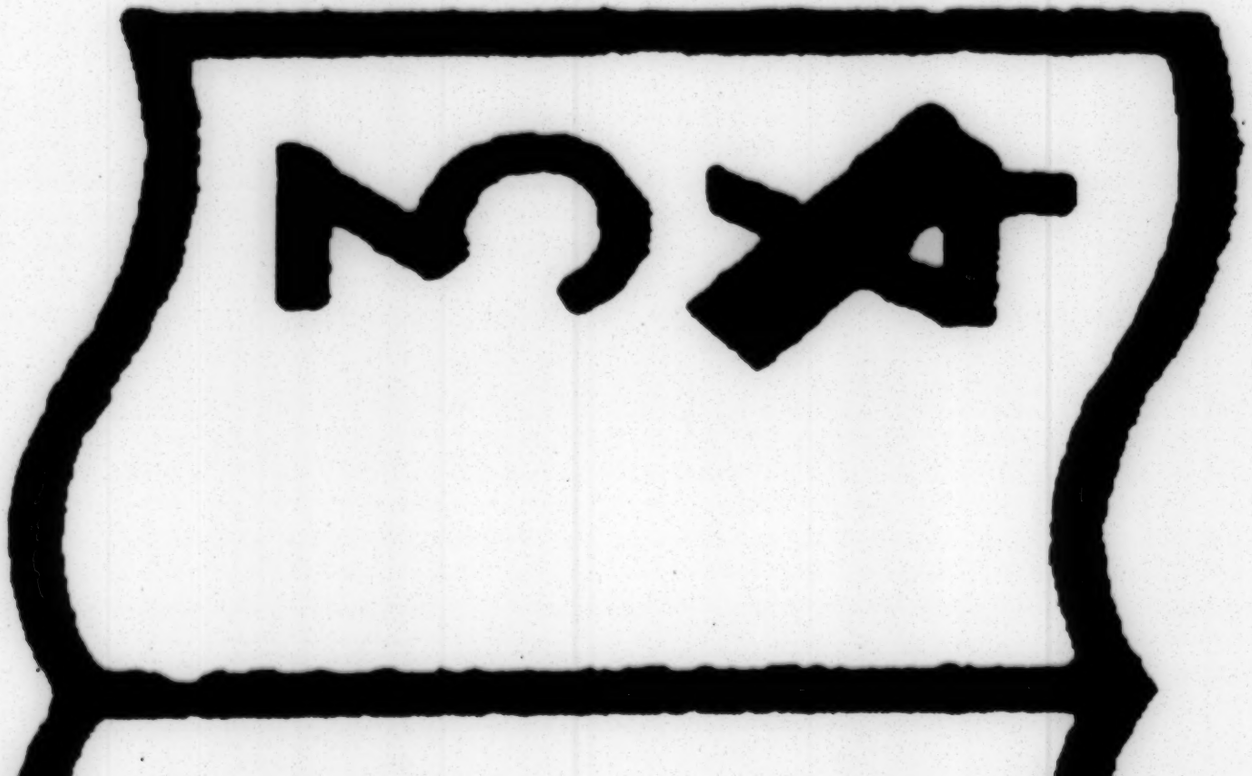
What was left after the foregoing ſeparation, was no longer acted upon by the nitrous acid; but being		One dram of each of the marles being calcined, weighed			The calcined marles put into water, produced
Mixed with water, became	When burnt	Grains.	Loſt grains.	Burnt to	
Uniform and plaſtic.	A hard red brick.	52	8	Red brick.	No effect.
Uniform and plaſtic.	A hard red brick.	53	7	Red brick.	No effect.
Plaſtic, but a little gritty.	A ſoft yellowiſh brick.	49	11	Soft yellow brick.	Weak lime water.
Uniform and plaſtic.	A hard red brick.	50	10	Red brick.	No effect.
Plaſtic.	A ſoft pale red brick.	48	12	Hard grey ſtone.	Lime water.
Gritty, no union.	No union.	51	9	Soft and ſtony.	Lime water.
No union.	A little coheſion.	46	14	Soft ſtone.	Lime water.
Plaſtic, but a little gritty.	A ſoft red brick.	48	12	Soft ſtone.	Strong lime water.
No union, gritty.	No union.	43	17	Soft ſtone.	Strong lime water.
No union, gritty.	No union.	48	12	Soft ſtone.	Strong lime water.
No union, gritty.	No union.	40	20	Soft ſtone.	Strong lime water.
Uniform and plaſtic.	A ſoft whitiſh brick.	29	31	Powdery.	Strong lime water.

added to each glaſs, they all effervеſced; N° 1 and 2 the leaſt, N° 12 the moſt. The effervеſcence having after filtration, changed violet paper to a red colour. To the filtered colours was gradually added mild fixed itated earth being waſhed in rain water, till free from all ſaline matter, weighed, when dry, as in column the ed in N° 1, 2, 4, a red clay; in N° 12 a white clay; in N° 8 a red clay, and a portion of ſand; in N° 3 a h a ſmall portion of clay. Theſe reſiduums were all waſhed with rain water before they were burnt. The pre- a ſtrong heat, loſt 35 grains in weight. Rain water was poured upon the calx; the next morning there was a upon the addition of mild fixed alkaly. The marles were kept for ſome weeks in a dry place before they were 12, which is found near the Duke of Bridgewater's canal, in a powdery form, and when mixed with one fourth ed to the weather.

iſeable to attempt burning the marles of this country into quicklime, for the purpoſes of agriculture; they may different kinds of lands. Perhaps the calcarious earth united with clay, as in N° 1, 2, 4, &c. may be the the moſt eligible where the land is already ſtiff, and abounding with clay. How far the different quantities of h, will influence their preference in agriculture, muſt be left to the experience of the farmer to determine.

Received


2



Received Nov. 12, 1772.

XX. *A Letter from* Patrick Brydone, *Eſq*; *to Sir* John Pringle, *Bart. Preſ. R. S. containing an Account of a fiery Meteor, ſeen on the* 10*th of* February *laſt*; *and alſo of ſome new electrical Experiments. Dated* Eccles, *(in the Shire of* Berwick) 7 July, 1772.

SIR,

Read Feb. 4, 1773. ABOUT fourteen years ago, I took the liberty of communicating to you ſome electrical experiments; which, you thinking worthy of the attention of the Royal Society, were pleaſed to lay before that reſpectable body; and thoſe gentlemen did me the honour of publiſhing them in the firſt and ſecond part of the fiftieth Volumes of the Philoſophical Tranſactions. Since that time, I have often been engaged in operations of the ſame kind, ſome of which, particularly the experiments with the electrical kite, I thought to have troubled you with; but as theſe were made in foreign countries, and as I was informed that ſomething of the ſame kind had been done in England, I ſuſpected that you might al-

ready have been acquainted with them; and this conſideration prevented me. However, laſt winter, on my return to this kingdom, I obſerved ſome facts in electricity, which I flatter myſelf you will not think undeſerving of your notice. What led me to make theſe experiments, was the ſtrong electrical appearances that the air exhibited during the laſt great froſt, and the obſervation of ſeveral meteors, and other phænomena, that poſſibly depend on electrical cauſes. One of theſe meteors was ſo remarkable, that I muſt beg leave to give you ſome ſhort account of it.

On Monday the 10th of February laſt, exactly at ſeven in the evening, as I was riding through Tweedmouth, a village at the ſouth end of Berwick-bridge, I obſerved that the atmoſphere was ſuddenly illuminated in a very extraordinary manner. The light of the moon, which was about half full, ſeemed to be extinguiſhed by the blaze; and I ſaw my ſhadow projected on the ground, and almoſt as diſtinct, and well-defined, as in ſunſhine. I turned round to ſee from whence the light proceeded, when I beheld a long, bright flame, moving almoſt horizontally along the heavens. It was of a conical form, and from the baſe to the apex could not be leſs than ſix or ſeven degrees; its height, when I firſt obſerved it, ſeemed to be about fifty degrees; but it deſcended gently, and appeared to burſt about five or ſix degrees lower. Its courſe was from north-weſt to ſouth-eaſt, and ſeemed to have an inclination to the horizon; but this might be only a deception. The baſe of the cone was rounded like a ſphere;

and

and apparently of about one third of the diameter of the moon at her greateſt height; but its light was brighter than that of the planet Venus, and in colour reſembled the flame of burning camphire. Near the end of the tail there was a kind of waving motion, which with the whole appearance, I have endeavoured to repreſent by this figure ——



In about ten or twelve ſeconds it ſeemed to burſt, dividing into a number of ſmall luminous bodies, like the ſtars in a ſky-rocket, which immediately diſappeared.

As I had formerly obſerved exploſions from meteors of this kind, I had preſence of mind to pull out my watch (which has a ſecond hand) to meaſure the exact time the report ſhould take in reaching me. I waited for upwards of four minutes, which in my ſtate of expectation appeared a much longer time; when, deſpairing of any report, I rode on, but had not got to the middle of the bridge, when I was ſtunned by a loud and heavy exploſion, reſembling the diſcharge of a large mortar, at no great diſtance, and followed by a kind of rumbling noiſe, like that of thunder. I examined my watch, and found, that the ſound had taken five minutes, and about ſeven ſeconds, to reach me; which, according to the common computation of 1142 feet in a ſecond, amounts to the diſtance of at leaſt 66 miles. It did not occur to me to meaſure the duration of the light,

which probably did not exceed ten or twelve ſeconds; and during this ſhort period, the length of the path, the meteor ſeemed to deſcribe, could not be leſs than 30 degrees. I expected to have ſeen ſome account of this phænomenon from Newcaſtle, as, by its direction and diſtance, I imagined it had burſt pretty near to the zenith of that city; but I have found no notice taken of it in the news-papers there. About a week after, I mentioned what I had ſeen to Sir John Paterſon, of Eccles, who told me he was at that time on the road, betwixt Greenlaw and his own houſe, in company with Mr. Thomas Cockburn, of Edinburgh; and, as they were riding to the ſouth, they obſerved the meteor from its firſt appearance, which was about three or four ſeconds ſooner than I had time to turn about and view it; and this, perhaps, is the reaſon that it appeared ſo much higher to them than it did to me. Theſe gentlemen obſerved, that when it firſt became luminous, it was almoſt vertical, but went off deſcending to the ſouth-eaſt, and had in other reſpects the appearance I have deſcribed. They added, that ſome conſiderable time after the light diſappeared, they heard a great report, which they took for a clap of thunder; for the interval was ſo long, that they did not imagine this ſound had any connection with what they had ſeen.

Now, as theſe gentlemen were at leaſt 20 miles to the weſt of the ſpot where I made my obſervation, and as the appearance and height of this meteor ſeems to have been nearly the ſame to them as to me, it is probable that it was at a very great

great diſtance from the earth, and much beyond the limits that have been aſſigned to our atmoſphere. The ſmaller meteors, which we call falling ſtars, I have frequently obſerved from the mountain of St. Bernard, one of the high Alps; and laſt year I had the good fortune to ſee ſeveral of them from the higheſt region of Mount Etna; an elevation ſtill more conſiderable, and probably the greateſt acceſſible one in Europe, and they always appeared as high, as when ſeen from the loweſt grounds; ſo that probably the height of two or three miles, bears but a ſmall proportion to the common altitude of theſe bodies.

From their frequent appearance, during the laſt froſt, I was inclined to believe, that the air was then in a very favourable ſtate for electrical purpoſes; but not being provided with a common machine, I bethought me of a whimſical one to ſupply the want of it. The back of a cat, it is well known, often exhibits ſtrong marks of electricity; being, therefore, deſirous to try what effect this might produce, when made uſe of inſtead of the glaſs globe, I cut a quantity of harpſichord-wire into ſhort pieces, of five or ſix inches, and tying them together at one end, made the other diverge like the hair of a bruſh. I took a large metal peſtle of a mortar for my conductor, to the end of which I fixed the bruſh of wire; and inſulated the whole, by placing it on a couple of wine-glaſſes. I then took a cat on my knee, and bringing her back under the wires, I began to ſtroak it gently. The animal continued in good humour for a few minutes, and I had the ſatisfaction

faction to ſee that the conductor was ſo much charged, that it emitted ſparks of a conſiderable force, and attracted ſtrongly ſuch light bodies as were brought near it; but the cat at laſt becoming uneaſy, threatned to put an end to our experiment. The paſſage of the electrical fire, from the hair of her back to the ſmall wires, occaſioned, it ſeems, a diſagreeable ſenſation, which ſhe could not bear; ſo that turning about her head to defend her back, the tip of her ear happening to touch the conductor, and a large ſpark coming from it, ſhe ſprung away in a fright, and would not allow me to come near her more. However, after a long interval, the animal ſeeming to have forgotten her adventure, a young lady in company, leſs obnoxious to her than I was, undertook to manage her. Having firſt covered the back of this lady's hand with a piece of dry ſilk, that none of the electric fire communicated to the wires might be loſt, ſhe then began to ſtroke the cat as I had done, and the conductor ſoon after appeared fully charged: we drew large ſparks from it, and if the animal would have continued quiet, I have no doubt that we ſhould have ſhewn many of the common experiments in electricity; but ſhe ſoon became ſo outrageous, that we were glad to put an end to our operations, without any hopes of being able to repeat them, at leaſt with the ſame inſtrument. In this dilemma I recollected, that a lady had told me, that on combing her hair, in froſty weather, ſhe had often been ſenſible of a little crackling noiſe; and in the dark had ſometimes obſerved ſmall ſparks of fire to iſſue from it. I propoſed, therefore,

therefore, that one of the young ladies would ſuffer the experiment to be made upon her head, which ſhe agreed to. The conductor was then inſulated as before, and the lady having placed herſelf ſo, that the back part of her head almoſt touched the bruſh of wire, I deſired her ſiſter to ſtand behind her, on a cake of bees-wax; who, as ſoon as ſhe began to comb the hair of the former, the conductor emitted ſparks ſtill of a larger ſize than thoſe we had hitherto ſeen. The hair was extremely electric, and when the room was darkened, we could perceive the fire paſs from it along the ſmall wires to the conductor. The young lady who was on the wax, was not a little ſurprized to find, that the moment ſhe began to comb her ſiſter's hair, her own body became electric, darting out ſparks of fire againſt every ſubſtance that approached her. We found, however, that theſe ſparks were not ſtrong enough to fire ſpirits. I then coated a ſmall phial, and ſoon charged it from the conductor; but afterwards I did it more compleatly from the hair itſelf in the following manner. I fixed a bruſh of ſmall wires to the large one that went through the cork of the phial; and taking the phial in my hand, I followed every motion of the comb with the bruſh of wires; and, in the dark, could obſerve the fire paſs by theſe wires into the bottle. In a few minutes I found it was highly charged; when taking a ſpoonful of warm ſpirits in my left hand, and with my right, which graſped the phial, bringing the hook of the great wire near the ſurface of the ſpirits, a large ſpark darted from it,

gave

gave me a ſmart ſhock, and at the ſame time ſet the ſpirits on fire.

The day following, we wanted to repeat our experiments; but as the weather was hazy, and the froſt had greatly abated, they did not ſo well anſwer. However, from making them on ſeveral heads, we found that the ſtronger the hair, the greater was the effect; whereas, ſoft flaxen hair produced little or no fire at all.

It may not be improper to mention, that theſe experiments were made in a warm, dry room, before a good fire, and at a time when the thermometer, in the open air, was at ſix or ſeven degrees below the point of congelation. The hair, which ſucceeded beſt, was perfectly dry, and no powder or pomatum had been uſed on it for ſome months before.

I have the honour to be,

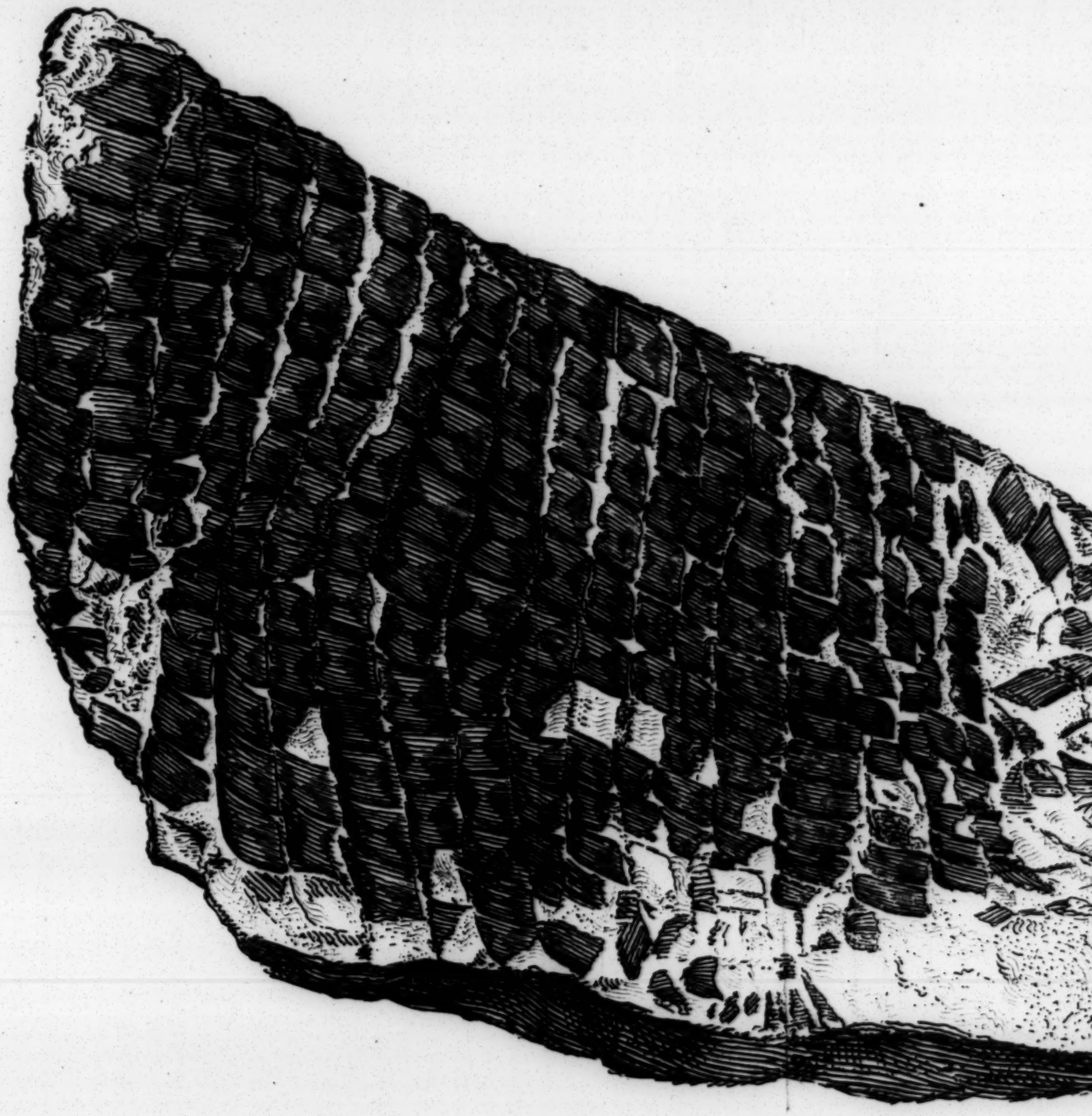
S I R,

Your moſt obedient,

and moſt humble ſervant.

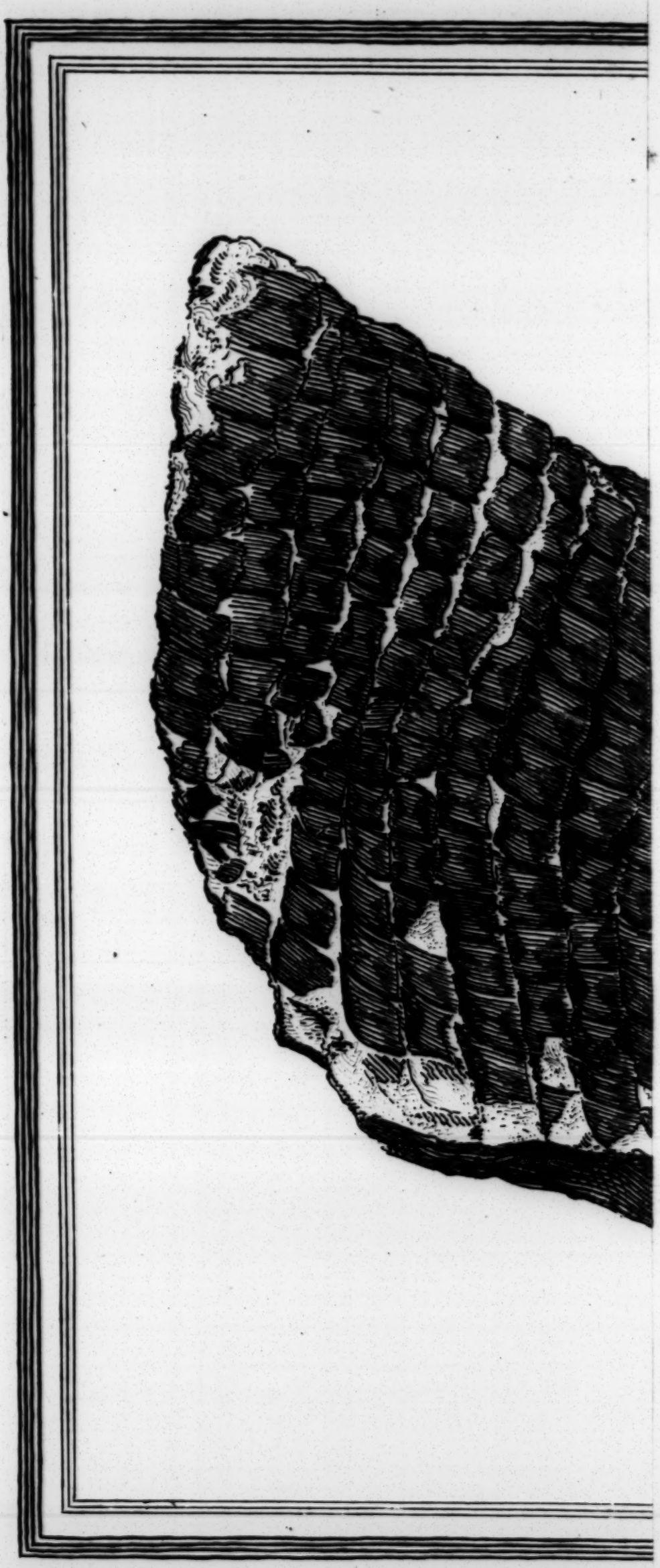
Patrick Brydone.

XXI. *Some*





Bauer del.



XXI. *Some Account of a Foſſil lately found near* Chriſt-Church, *in* Hampſhire; *in a Letter to Dr.* Maty, *Sec. R. S. from the Hon.* Daines Barrington, *Vice-Preſ. R. S.*

DEAR SIR,

Read Feb. 4, 1773. I Send herewith a foſſil which was found in a cliff, near Chriſt-Church, in Hampſhire, by the Rev. Mr. Howel, of Pool, and which Mr. Gough, F. A. S. deſires may be ſhewn to the Royal Society, if it ſhould be thought worthy their attention. [See Tab. VII.]

I am very little verſed in this branch of natural hiſtory; but as I am the unworthy channel of communication, I thought it my duty to procure the beſt information I was able, with regard to this very ſingular ſpecimen.

The ſhining diviſions, upon the ſurface of the ſtone, ſeem to be the ſcales of a fiſh, which I ſhould conceive to be the *Acus maxima ſquamoſa*, engraved in Willoughby's Hiſtory of Fiſh, Tab. p. 8. and deſcribed by Ray, in his Synopſis Piſcium, p. 109.

It appears by the catalogue of Engliſh foſſils, in the collection of Dr. Woodward, that a ſtill larger ſpecimen of the ſame ſort was found in Stansfield quarry, near Woodſtock, though Dr. Woodward could only procure a ſingle ſcale. V. II. p. 53. c. 24.

Single ſcales from the ſame quarry, are alſo to be ſeen in the noble collection of foſſils, given by Mr. Brander, F. R. S. to the Britiſh Muſeum.

Though this fiſh, therefore, is a ſtranger to our ſeas, yet its exuviæ are by no means ſo to our cliffs and quarries.

I am,

Dear SIR,

Your moſt faithful

humble ſervant,

Daines Barrington.

P. S. Mr. Hunter, F. R. S. having ſeen the foſſil at Crane-Court, happened to diſſect a beaver's tail very ſoon afterwards, which he ſhewed me, as bearing a ſtrong reſemblance to the ſcaly diviſions in this ſpecimen; I cannot, however, but ſtill think that the form of the ſcales in the *Acus maxima ſquamoſa* of Willoughby is ſtill nearer to it, than of thoſe in a beaver's tail.

XXII. *A Deſcription of a rare* American *Plant of the* Brownææ *Kind; with ſome Remarks on this Genus. By Mr.* Peter Jonas Bergius, *F. R. S.*

Read Feb. 11, 1773.

AS the *Leucandendra*, *Bruniæ*, *Dioſmæ*, *Phylicæ*, *Hermanniæ*, &c. are peculiar to Africa, ſo are likewiſe the *Varroniæ*, *Ehretiæ*, *Samydæ*, *Malpighiæ*, *Cacti*, *Brownææ*, &c. peculiar to America, not having been found in any other country: at preſent I will confine myſelf to the laſt mentioned kind.

Mr. Jacquin, during his botanical travels in America, founded this genus, in memory of Dr. Patrick Browne, the celebrated Engliſh botaniſt; but Jacquin found only one ſpecies of this genus; neither was Sir Ch. Linné hitherto acquainted with any more.

I have now ſpecimens of a new ſpecies of this kind, which I received from Mr. Pihl, who gathered it in Portobello in America, which will afford an opportunity of exhibiting the whole genus of the *Brownææ*, and the ſpecifical differences of it.

If we compare Mr. Jacquin's deſcription of his ſpecies with mine, we ſee how carefully nature has obſerved the ſame order and poſition of the eſſential

parts in both; a circumſtance common to all natural genera.

I don't know whether this plant will vegetate and thrive in our ſtoves or green-houſes; if it does, I am convinced it will make a beautiful appearance with its aſſemblage of purple or blood-red flowers.

The drawings which I herewith ſend [TAB. VIII. and IX.], ſhew a ſtalk with a flower, and one without, ſo that the receptacle is bare.

BROWNÆÆ Genus.

1. BROWNÆA (*coccinea*) floribus disjunctis umbellatis.

Brownæa coccinea. Linn. Spec. Plant. 958. Jacquin. Hiſt. Stirp. Americ. 194. * Tab. 121.

Habitat in rupeſtribus ſylvaticis Zaucæ ad ſinum Venezuelæ Americes.

Deſcr. apud Jacquinum, loco citato.

2. BROWNÆA (*Roſa de monte*), floribus aggregato-capitatis ſeſſilibus, ſtaminibus longiſſimis.

Hermeſias. Loefling. Itin. p. 278. *

Habitat in montoſis Terræ Firmæ, Portobello.

DESCR. *Caulis* arboreus.

Rami toruloſi, cortice cinereo.

Ramuli (ſeu petioli communes) ſubalterni, teretes, glabri, baſi geniculo ſuberoſo-rugoſo, patentes.

Folia coriacea, ſpithamæa, oppoſita, ovato-oblonga, integerrima, longius acuta, utrinque glabra, nervis alternis obſoletis, breviter petiolata; inferiora ſenſim minora; infima ovata, baſi ſubcordata.

Petiolelli

Brownæa (Rosa de



de Monte.)



Petiolelli breves, craſſi, rugoſi.

Flores intra calycem communem aggregati in capitulum ſ. faſciculum ſubrotundum, ſpecioſiſſimum, magnitudine pugni.

Faſciculi ſolitarii, alterni, diſtantes, ſeſſiles, ſubaxillares.

Calyx communis imbricatus: foliolis ſ. bracteis ovatis, acutiuſculis, ſubmembranaceis, concavis, laxiuſculis, glabris, bipollicaribus, rubicundis; ſingulis includentibus ſingulos vel etiam 2 aut 3 flores; deciduis; exterioribus rotundatis; interioribus minoribus, ſenſim linearibus.

Perianthium proprium cylindricum, tubulatum, ſuperne paululum ampliatum, rubicundum, villoſum, bifidum: laciniis ovatis, acutiuſculis, ſubæqualibus, erectis.

Corolla univerſalis uniformis, ſanguinea.

Propria duplex.

Exterior infundibuliformis, calyce longior: tubus cylindricus, ſubangulatus, deorſum anguſtatus, ſubcoriaceus, perſiſtens; limbus 5-fidus (ſæpe 4-fidus): laciniis lanceolatis, obtuſis, erectis, inæqualibus, unica duplo latiore, deciduis.

Interior pentapetala: petalis ovato-lanceolatis, obtuſis, latiuſculis, erectis, corolla exteriore fere duplo longioribus: unguibus ſubulatis, margini tubi corollæ exterioris inſertis.

Stam.

Stam. Filamenta conſtanter undecim, filiformia, longiſſima, h. e. corolla duplo longiora, erecta, ſubcurvata, æqualia, inferne coalita in tubum integrum, antice hiantem, germen cingentem, margini tubi corollæ exterioris adnatum, dein in filamenta, baſi æqualia, fiſſum.

Antheræ ovatæ, incumbentes.

Piſtill. Germen ſuperum, è tubo corollæ exterioris pedunculatum: pedunculo tubo adnato; cylindricum, tomentoſum.

Stylus filiformis, longitudine ſtaminum, inflexus.

Stigma ſimplex.

Peric. Legumen oblongum, compreſſum, circa diſſepimentum anguſtatum, plerumque biloculare.

Diſſepimento membranaceo.

Semina ſolitaria, ovata, compreſſa, rugoſiuſcula, fibris fungoſis obvoluta.

Received

Philos. Trans. Vol. LXIII. Tab. IX *p. 176.*



I.^s Basire Sculp.

Philos. Trans. Vol. LXIII. Tab. IX p. 176.

Bronnaea (Rosa de Monte.)



W. Bauer Sculp.

Received Nov. 12, 1772.

XXIII. *Extract of a Letter from the Rev.* Samuel Kirkſhaw, *D. D. Vicar of* Leeds, *and Rector of* Ripley, *in the County of* York, *to* William Mountaine, *Eſq; F. R. S. Communicated by* William Watſon, *M. D. and F. R. S.*

Read Feb. 11, 1773.

ON the 29th of Sept. laſt, about two o'clock in the morning, were three remarkably loud claps of thunder, attended with proportionable lightning. Mr. Thomas Heartly, formerly Wine-merchant, of Leeds, but lately retired from buſineſs to Harrowgate, lived there in a hired houſe, the ſecond northward from the Queen's-head. While he was in bed with his wife, ſhe was awaked from ſleep by the thunder, and went to the window; but not being afraid, ſhe got to bed again, and fell aſleep. About five ſhe awaked; and, not perceiving her huſband to breathe, though warm, endeavoured to awake him—in vain! She quickly ſent for Mr. Hutchinſon, a conſiderable apothecary at Knareſborough, who, upon ſight of Mr. Heartly, and ſome experiments, declared him dead, though ſtill very warm. At her requeſt,

requeſt, however, he opened a vein; and Mr. Heartly bled freely, inſomuch that the blood did not ceaſe to ooze out of the orifice till the body was put in the coffin, which was on Thurſday evening, the firſt inſtant, viz. October, and it was not even then cold. His hair, which he wore, was conſiderably burnt, or ſinged on the right ſide of his head, which was uppermoſt (for he lay then on his left ſide) and the inſide of his night-cap, on the ſame ſide, was ſinged or browned, though no where on the outſide marked at all. Within the cap was found a ſplinter from the bed-poſt next to his head, which poſt was torn and ſplit into many ſplinters or ſhivers, from the top to the bottom, though a ſtrong oaken poſt, and almoſt new. No wound, or mark of any ſort, was diſcovered on any part of his body; but the lower part of his right cheek was ſwelled, and much hardened. So far I was told by Mrs. Heartly and her ſervants, but I ſaw the bed-poſt.

In the chamber where this happened, there was a ſmall chimney to the north, made up, but not quite cloſe, by a chimney-board, upon which I could not diſcover any mark or hole, or other indication of the lightning's paſſing that way.

Between that chimney and the weſt end of the room, ſtands the bed, in the north-weſt corner of the room, cloſe to the weſt and north walls; the deceaſed lay next to the weſt wall, with his head near to the head bed-poſt, in the north-weſt corner aboveſaid.

There is only one window in the room, full eaſt, conſiſting of three pretty large lights, ſepa-

rated by two ſtone mullions, each light ſupported by ſix ſtrong iron bars acroſs it, parallel to the floor, and the intermediate one, rather more than half of it, made into a caſement, the frame of which is of iron, and the ſurrounding frame of the ſame. In the ſouthermoſt light, which had three ſquares of glaſs in breadth, two of the loweſt ſquares were perforated in or near the middle, about an inch ſquare; but as ſome ſmall parts of the glaſs were gone, I could only gueſs at the ſize of the holes, nor could diſtinctly eſtimate the ſhape of them, nor form the ſlighteſt conjecture, whether the lightning had made its ingreſs or egreſs through both, or either of them. The intermediate ſquare of glaſs left perfectly ſound—there was no other iron about the window, except the abovementioned—but the curtain-rods of the bed, which ſtood about ten feet from the window, I obſerved, were iron, ſtronger (larger) ſomewhat than uſual.

Mrs. Heartly lay on Mr. Heartly's left hand, when the thunder was, and felt not the leaſt ſtroke from the lightning, or perceived any effects from it, except that her right arm, ſhe found, when ſhe awoke, was ſtunned and benumbed, and a little painful, which continued for a few days, but is now quite well.

I took notice of a pump, which ſtood about ten or eleven feet from the houſe, in nearly a right line from the window abovementioned, whoſe handle (or ſreape, as it is called hereabout) is all of iron, very thick and long, and a ſtrong iron ball for a head to it.

Ripley, Oct. 12, 1772.

XXIV. *Extract of a Letter from* Paul Panton *Esq; of* Plaſwgyn *in* Angleſey, *to the Honourable* Daines Barrington, *V. P. concerning the Increaſe of Population in* Angleſey.

Plaſwgyn in Angleſey, Oct. 3, 1772.

DEAR SIR,

Read Feb. 25, 1773. I Wiſhed to have ſent you a fuller account of the ſtate of population in this iſland; but ſo little care has been taken to preſerve the pariſh regiſters, that ſcarcely any that are antient are to be met with. Upon the whole of thoſe I ſend you, there is great reaſon to make a pleaſing concluſion, that we become more healthy, and increaſe in population. Heretofore the inhabitants of this iſland lived chiefly upon fiſh, with which, eſpecially herrings, theſe coaſts were abundantly furniſhed. Salted herrings were their principal food. This rambling fiſh, the herring, having left us, our iſlanders have neglected purſuing other branches of the fiſhery, and have betaken themſelves more to agriculture. The potatoe plant has not been cultivated in any great quantities here until of late years; but, ſince the failure of our herring fiſhery, it has made great part of the food of the inha-

inhabitants. Perhaps the want of the one, and the increaſed conſumption of the other, may be amongſt the cauſes that have contributed to the better health of our people. The increaſe in population in Llanduvnan and Pentraeth pariſhes, has not been owing to mines, or any new advantage introduced. The inhabitants are wholly employed in huſbandry.

I am,

Dear SIR,

Your very obliged humble ſervant,

Paul Panton.

BIRTHS and BURIALS, in LLANSADURN Pariſh, Angleſey.

	Chriſtened.	Buried.		Chriſtened.	Buried.
1590	6	2	1620	5	5
91	1	4	21	3	4
92	—	2	22	7	7
93	5	5	23	1	6
94	5	4	24	6	1
95	7	3	25	3	2
96	5	3	26	4	6
97	5	7	27	5	2
	34	30		34	33

BIRTHS, &c. in LLANSADURN Parish.

	Christened.	Buried		Christened.	Buried.
1750—	8	— 6	1764—	9	— 10
51—	5	— 4	65—	10	— 1
52—	6	— 3	66—	9	— 7
53—	10	— 12	67—	6	— 10
54—	9	— 4	68—	11	— 8
55—	9	— 8	69—	9	— 11
56—	9	— 8	1770—	10	— 16
57—	9	— 3	71—	5	— 5
	63	—50		69	—68

BIRTHS, &c. in PENTRAETH Parish.

	Christened.	Buried		Christened.	Buried.
64—	11	— 14	1672—	11	— 10
1665—	7	— 10	73—	8	— 8
66—	6	— 6	74—	10	— 11
67—	6	— 14	75—	13	— 11
68—	8	— 5	76—	10	— 25
69—	8	— 37	77—	19	— 8
70—	7	— 48	78—	19	— 12
1671—	9	— 43	79—	12	— 21
	69	—188		102	—106

	Weddings.	Christenings.	Burials.		Weddings.	Christenings.	Burials.
1740—	2	— 11	— 8	1764—	5	— 20	— 10
41—	2	— 15	— 17	65—	3	— 22	— 7
42—	7	— 6	— 9	66—	1	— 14	— 9
43—	4	— 14	— 18	67—	5	— 19	— 15
44—	8	— 14	— 10	68—	5	— 18	— 11
45—	2	— 12	— 7	69—	2	— 18	— 15
46—	2	— 14	— 5	1770—	8	— 14	— 11
47—	5	— 14	— 11	71—	4	— 24	— 12
	32	—100	—85		33	—149	—80

BIRTHS, &c. in LLANVAIR yn GORNWY Pariſh.

	Chriſtened.	Buried.		Chriſtened.	Buried.
1732—	10	— 13	1764—	12	— 18
33—	7	— 7	65—	8	— 11
34—	12	— 6	66—	16	— 5
35—	9	— 5	67—	10	— 15
36—	6	— 17	68—	10	— 8
37—	10	— 7	69—	19	— 5
38—	5	— 5	1770—	14	— 9
39—	9	— 7	71—	12	— 6
	68	—67		101	—77

BIRTHS, &c. in BEAUMARIS Pariſh.

	Chriſtened.	Buried		Chriſtened.	Buried		Chriſtened.	Buried.
1676—	19	— 19	1710—	18	— 26	1764—	44	— 22
77—	15	— 17	11—	26	— 29	65—	44	— 28
78—	18	— 19	12—	26	— 21	66—	41	— 38
79—	18	— 16	13—	22	— 17	67—	32	— 35
1680—	14	— 24	14—	45	— 24	68—	44	— 28
81—	16	— 23	15—	31	— 36	69—	37	— 47
82—	14	— 25	16—	33	— 21	70—	47	— 22
83—	18	— 31	17—	35	— 38	71—	39	— 29
	135	—174		236	—212		328	—249

BIRTHS,

BIRTHS, &c. in LLANDDYFNAN Parish.

	Weddings.	Christenings.	Burials.
1547	—	5	4
48	1	4	3
49	—	8	3
1550	1	7	6
51	1	—	6
52	—	3	2
53	4	3	5
54	1	6	10
	8	36	39

	Weddings.	Christenings.	Burials.
1620	3	2	2
21	2	4	1
22	1	1	7
23	3	6	21
24	1	4	18
25	4	7	8
26	4	12	5
27	2	8	5
	20	44	67

	Weddings.	Christenings.	Burials.
1750	1	7	5
51	1	9	4
52	9	15	6
53	6	23	11
54	4	9	11
55	2	18	4
56	2	14	2
57	3	16	3
	28	111	46

	Weddings,	Christenings.	Burials,
1764	7	18	14
65	5	19	10
66	3	26	14
67	2	17	17
68	2	20	13
69	7	24	24
1770	4	16	12
71	2	14	4
	32	154	108

XXV. *A Letter to the Rev.* Nevil Maſkelyne, *F. R. S. Aſtronomer Royal, from Mr.* Bailly, *of the Royal Academy of Sciences at* Paris: *Containing a Propoſal of ſome new Methods of improving the Theory of* Jupiter's *Satellites. Tranſlated from the* French, *with the Original underneath.*

SIR,

Read Feb. 18 and 25, 1773.

THOUGH I have not the honour of being perſonally known to you, I flatter myſelf, you will excuſe the liberty I have taken, of communicating to you two methods, of my invention, for perfecting the theory of the ſatellites of Jupiter. The former of theſe methods ſerves to meaſure their diameters, and the latter is intended to make the obſervations comparable with each other, though made in different places, and with different inſtruments. As my intention has been

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Monſieur,

Quoique je n'aye pas l'honneur d'être connu de vous, je penſe, que vous ne trouverez pas mauvais, que je prenne la liberté de vous communiquer deux méthodes, que j'ai imaginées, pour perfectionner la theorie des ſatellites de Jupiter. La première de ces méthodes ſert à meſurer leur diametre, et la ſeconde a pour objet de rendre les obſervations comparables, ſoit quelles aïent été faites en differens lieux et avec differens inſtruments.

Mon

been to make theſe methods uſeful, I cannot do better, than ſubmit them to the beſt judges. Your reputation, Sir, engages me, to lay them before you, and to beg you would examine them: flattered as I ſhould be with your approbation, I ſhall receive, with great gratitude, whatever criticiſms they may happen to ſuggeſt. Obſervation muſt determine, whether, by this method, it will be poſſible to arrive at an agreement, which hitherto has been wiſhed for, without being attained.

You know, that the obſervations of the eclipſes of the third and fourth ſatellites, made by different obſervers, vary from each other 3, 4, and 5 minutes, and ſometimes more; and that there is even a pretty ſenſible difference in thoſe of the ſecond. In the 38th page of the preface to my *Eſſay on the Theory of the Satellites*, which has been preſented to the Royal Society, I mentioned the inequality diſcovered by Mr. de Fouchy, and I ſuggeſted, that the perfecting

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Mon bût a été que ces méthodes ſoient utiles; je ne puis mieux faire, que de les ſoumettre aux meilleurs juges. Votre réputation, Monſieur, m'engage à vous les détailler, et à vous prier de les examiner: votre ſuffrage me flatterait infiniment, mais je recevrai toujours avec reconnoiſſance les reflections critiques que ces méthodes pourront vous ſuggérer. Les obſervations vous feront connoitre ſi par cette méthode on peut parvenir à un accord que l'on a ſouhaité juſques ici ſans l'avoir obtenu.

Vous ſavez, que les éclipſes du troiſième et du quatrième ſatellite, faites par differens obſervateurs, different entr'elles de 3, 4, 5 minutes et quelquefois plus; et que celles du ſecond different même aſſez ſenſiblement. Dans *L'Eſſai ſur la Théorie des Satellites*, dont j'ai fait hommage a la Societé royale, je parlais dans la préface, page xxxviii, de l'inégalité découverte par Mr. de Fouchy, et j'annonçais,

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ing of this theory might perhaps depend upon the quantity of this inequality, which Mr. de Fouchy has not determined, not having been at leiſure, to reſume the ſubject, ſince the year 1732. The ſegment of the diſc, which is not eclipſed, when the ſatellite diſappears, muſt vary in the proportion of the ſquares of the diſtances of Jupiter from the Sun, and from the Earth. This is what a little reflection will make evident to every one, and this is the firſt cauſe of the inequality. Since Mr. de Fouchy's obſervation, it has been diſcovered, that the light of the ſatellite likewiſe decreaſes, in proportion to the proximity of Jupiter's diſc; the brightneſs of the planet weakens that of the ſatellite, and, for this reaſon, the eclipſes, which happen too near the oppoſition [of Jupiter to the Sun], are looked upon as defective. Beſides, the light of Jupiter, as well as that of his ſatellites, is different, in his different

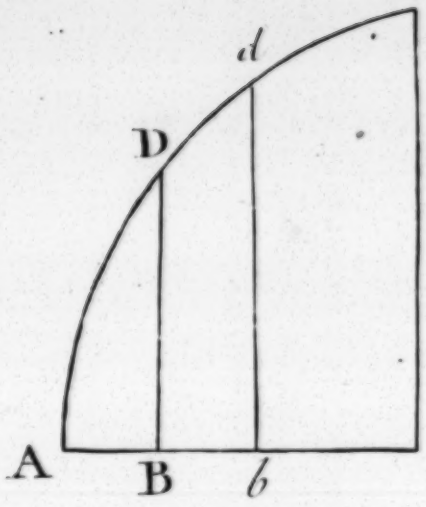
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nonçais, que la perfection de cette théorie dependrait peut être de la quantité de cette inégalité, que Mr. de Fouchy n'a point déterminée; il n'a pû s'en occuper depuis 1732. Il réſulte de la conſideration du ſegment du diſque, qui n'eſt pas éclipſé, quand le ſatellite diſparait, que ce ſegment varie en raiſon [*inverſe*] des quarrés de la diſtance de Jupiter au Soleil, et de la diſtance de Jupiter à la Terre. Voila la premiere cauſe d'inégalité. Depuis Mr. de Fouchy, on s'eſt apperçu que la lumiere du ſatellite diminuoit encore en raiſon de la proximité du diſque de Jupiter; l'éclat de la planete efface celui du ſatellite, c'eſt pour cette raiſon que les éclipſes, qui arrivent trop près de l'oppoſition, ſont regardées comme défectueuſes. De plus, la hauteur de Jupiter ſur l'horiſon contribuë auſſi à affoiblir ſa lumiere, et celle de ſes ſatellites;

different elevations above the horizon: when the planet is low, more rays of light are loſt, in their paſſage through a thicker atmoſphere; and whenever the light is leſs, the ſegment, which is not eclipſed when the ſatellite diſappears, and which I call the inſenſible ſegment, increaſes, and occaſions another inequality in the moment of the eclipſes; laſtly, the power of the teleſcopes, or their aperture, which, according as it is greater or leſs, gives more or leſs light, contributes to the variation of this ſegment. Here then are four cauſes of inequality, which I reduce to one principle, and the following is the ſcope of my reſearches. When the ſatellite diſappears, there is certainly a ſegment of its diſc which remains uneclipſed; the magnitude of this varies, on account of the four cauſes juſt mentioned; thence it follows, that

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tellites: quand cette planete eſt peu élevée, il ſe perd plus de raïons en traverſant une atmoſphere plus épaiſſe; or, toutes les fois que la lumiere diminuë, le ſegment, qui n'eſt pas éclipſé quand le ſatellite diſparait, ce ſegment que j'appelle le ſegment inſenſible, augmente, et il en réſulte une autre inégalité dans le moment des éclipſes; la force des lunettes, ou leur ouverture qui, lors qu'elle eſt plus ou moins grande, fournit plus ou moins de lumiere, contribuë encore à faire varier ce ſegment. Voila donc quatre cauſes d'inégalités, que je ramene à un même principe, et quant à mes recherches, voici ce dont il eſt queſtion. Quand le ſatellite diſparait, il y a certainement un ſegment de ſon diſque qui n'eſt pas éclipſé. La grandeur de ce ſegment varie en raiſon des quatre cauſes que nous venons d'établir. D'où il réſulte, que



that if in one eclipſe the ſegment is A B D, and in another A *b d*, when the ſatellite diſappears in the ſecond eclipſe, it will have got leſs into the ſhade, by a part of its diameter B *b*; which part B *b*, therefore, muſt be the value of the equation between the two eclipſes. Now, if we call A *b*, *a*; A B, *b*, the radius of the diſc of the ſatellite *r*, the ſemidiameter of the ſhadow taken from the tables R, and the total duration of the eclipſe *d*, the time taken up in going over B*b*, or the equation (*a*), will be $\frac{2Rr(a-b)}{d}$, which contains three unknown quantities, to wit, the verſed ſines *a* and *b*, of the two inviſible ſegments, and the ſemidiameter of the diſc of the ſatellite:

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que ſi pour une éclipſe ce ſegment eſt A B D, et que pour une autre éclipſe il ſoit A *b d*, dans cette ſeconde éclipſe, quand le ſatellite diſparaitra, il ſera moins entré dans l'ombre, d'une partie B *b* de ſon diametre, laquelle partie B *b* eſt la valeur de l'équation entre ces deux éclipſes. On trouve facilement que ſi l'on nomme A *b*, *a*; A B, *b*, le raïon du diſque du ſatellite *r*, le demi diametre de l'ombre tiré des tables R, et la durée entiere de l'éclipſe *d*; le tems emploïé à parcourir B *b*, ou l'équation, ſera $\frac{2Rr(a-b)}{d}$. Cette expreſſion contient trois inconnuës, les ſinus verſes *a* et *b* des deux ſegmens inviſibles, et le demi diametre *r* du diſque du ſatellite. Car vous ſavez,

tellite: for you know, Sir, that there is nothing to be depended on, in all that has been done upon the diameters of the ſatellites by Caſſini, Whiſton, and Maraldi. The following is the way which I have taken, to determine theſe unknown quantities. I obſerve, firſt of all, that two of them, *a* and *b*, are reducible to one; becauſe, as you will ſee preſently, the two ſegments are always in a known proportion [to the whole diſc of the ſatellite, as well as to each other]; and conſequently, the proportion of their verſed ſines, A*b*, AB, may be obtained, either by calculation, or by a table made for the purpoſe. In order to diſcover it, conſidering that when the ſatellite diſappears, it is from the diminution of its light, I conceived, that one might contrive to imitate, at any time, what happens in the eclipſes, by diminiſhing the light. I have an achromatic teleſcope of five feet length, and 24 lines aperture. I made ſome diaphragms of paſteboard, which I could apply on the

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Monſieur, tout ce qui a été fait ſur les diametres des ſatellites par Caſſini, Whiſton, et Mr. Maraldi, et vous ſavez qu'il n'y a rien ſur quoi on puiſſe compter. Voici comment je ſuis parvenu à les déterminer. Je remarque d'abord que les deux inconnuës, *a*, *b*, ſe réduiſent à une ſeule, parceque, comme vous le verrez tout à l'heure, les deux ſegmens ſont toujours dans un rapport connu, et conſéquemment par le calcul ou par une table dreſſée exprès, on a le rapport de leurs ſinus verſes A*b*, AB. Pour les connaitre, j'ai vû que quand le ſatellite diſparait, c'eſt par la diminution de ſa lumiere, et j'ai penſé, qu'à tous les momens on pouvait imiter, d'une maniere factice, ce qui arrive dans les éclipſes, en diminuant la lumiere du ſatellite. J'ai une lunette achromatique de 5 pieds, qui porte 24 lignes d'ouverture. J'ai taillé des diaphragmes de carton, que je pouvais appliquer

the outſide of my object-glaſs, the openings of which leſſened, by half-lines ſucceſſively, from twenty-four lines down to three. In fine weather, I applied theſe ſuceſſively to my object-glaſs, and endeavoured to find out, whether, by trying from the greateſt to the leſs, ſome one of them could not be found, that would make the ſatellite diſappear. My ſucceſs in this gave me great ſatisfaction. One day, for inſtance, the third ſatellite diſappeared, when the opening was reduced to three lines, and the firſt, when it was reduced to ſix only; and as, in the teleſcopes, the quantity of light is in the proportion of the ſquares of the apertures, I concluded, that the 64th part of the light of the third ſatellite, and the 16th of the firſt, were inſenſible; whence it follows, that if, at the inſtant of an eclipſe of the firſt ſatellite, the ſixteenth part of its light is inſenſible, the inviſible ſegment A B D will be likewiſe a ſixteenth part of the diſc; and thence it will be eaſy to compute

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appliquer extérieurement ſur mon objectif, et dont les ouvertures diminuaient de demie ligne, depuis 24 lignes juſqu'à 3. Par un beau tems, j'ai placé ſucceſſivement ſur mon objectif tous ces diaphragmes, et j'ai cherché, ſi en paſſant toujours des plus grands aux plus petits, il n'y en aurait pas quelqu'un, qui fit diſparaitre le ſatellite; et j'ai reuſſi avec une grande ſatisfaction. Un certain jour, par exemple, le troiſième ſatellite diſparut, lorſque l'ouverture fut réduite à trois lignes, et le premier, lorſque l'ouverture fut réduite à ſix ſeulement. Comme dans les lunettes la lumiere eſt dans le rapport du quarré des ouvertures, j'en ai conclu que la 64me partie de la lumiere du troiſième, et la ſeizième du premier étaient inſenſibles : d'où il reſulte, que ſi dans le moment d'une éclipſe du premier ſatellite la ſeizième partie de ſa lumiere eſt inſenſible, le ſegment inviſible A B D ſera auſſi la ſeizième partie du diſque, d'où il eſt aiſé de calculer le ſinus verſe

compute the verſed ſine A B. In theſe firſt obſervations, I took care to chuſe the time, when the ſatellite was at its greateſt elongation; for the inſenſible part increaſes prodigiouſly, and ſometimes amounts to a third of the diſc, when the ſatellite is very near the edge of Jupiter. This variation is much larger, than that which takes place, in conſequence of the diſtance of Jupiter from the oppoſition to the Sun, and contrary to it. As it is ſcarce poſſible to eſtimate the law of the variations of this ſegment, occaſioned by the proximity of the diſc of Jupiter, I judged, that they ought to be determined by obſervation. Accordingly, I followed the ſatellite from the edge of the diſc of Jupiter, to the fartheſt limit of its eclipſes, that is, with reſpect to the firſt, to the diſtance of two ſemidiameters of Jupiter. Having thus ſeveral points by obſervation, I got the reſt by interpolation, and made a table of the variations of the inviſible ſegment,

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verſe A B. J'avais eu ſoin dans ces premieres obſervations, de choiſir le tems où le ſatellite était dans ſes plus grandes digreſſions. Car cette partie inſenſible augmente prodigieuſement, et devient quelquefois le tiers du diſque, lorſque le ſatellite eſt fort près du bord de Jupiter. Cette variation eſt contraire et beaucoup plus grande que celle qui a lieu en vertu de la diſtance à l'oppoſition. Comme on ne peut gueres eſtimer la loi que ſuivent les variations de ce ſegment, à raiſon de ſa proximité du diſque de Jupiter, j'ai jugé qu'il falloit déterminer ces variations par obſervation. J'ai donc ſuivi le ſatellite depuis le bord du diſque de Jupiter, juſqu'au terme le plus éloigné de ſes éclipſes, c'eſt à dire, à l'égard du premier, juſqu'environ à la diſtance de deux demi diametres de Jupiter. Ayant obtenu ainſi pluſieurs points donnés par obſervation, j'ai eu les autres par interpolation, et j'ai dreſſé une table des variations du ſegment inviſible, qui dépendent de la diſtance au bord

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ment, which depend upon the diſtance from the edge of Jupiter; a ſimilar table I likewiſe, made for each of the three firſt ſatellites; but have not yet been able, to make ſufficient obſervations on the fourth. Theſe tables are contained in a long paper of mine, which will be publiſhed in the volume of our Academy for 1771; but, if you pleaſe, I will ſend them to you. Theſe ſegments being known, it is clear, that, beſides their variations occaſioned by the diſtance of the ſatellite from the edge of Jupiter, they will be liable to others. Firſt, In conſequence of the change of Jupiter's diſtances, both from the Sun and from the Earth. Upon this account, the magnitudes of theſe ſegments being known, for a particular epoch *, thoſe known magnitudes muſt be multiplied by $\frac{m^2 n^2}{p^2 q^2}$, to determine the magnitude of the ſegment at any other time. In which expreſſion, p and q ſtand for the di-

* *Known*, by the author's tables, for any diſtance of the ſatellite from the edge of Jupiter, at the particular epoch to which the tables are adapted.

ſtances

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de Jupiter; j'ai dreſſé une pareille table pour chacun des trois premiers ſatellites; je n'ai pû obſerver encore, comme il faut, le quatrième. Si vous déſirez ces tables, Monſieur, je vous les envoyerai. Elles ſont dans un long mémoire, qui ſera imprimé parmi ceux de l'Academie pour 1771. Ces ſegmens étant une fois connus, il eſt clair, qu'outre les variations que ces ſegmens ſubiſſent, en vertu de la diſtance du ſatellite au bord de Jupiter, ils varieront encore. 1°. En vertu de la diſtance de Jupiter au Soleil, et de Jupiter à la Terre. Il faudra donc multiplier ces ſegmens, connus pour une certaine époque, par $\frac{mm\,nn}{pp\,qq}$, en ſuppoſant que p et q aient été les di-

ſtances

ſtances of Jupiter from the Sun and from the Earth reſpectively, at the given epoch, and *m* and *n* for the diſtances, at the other time, for which the value of the inviſible ſegment is required. 2dly, There will be other variations depending on Jupiter's height above the horizon. The ſegments which I have obſerved, have all been reduced to the conſtant height of 15°. Mr. Bouguer, in his Optics, has given a table of the degrees of light of the planets, at their different elevations above the horizon, which, from my own obſervations, I have found to be very exact, and uſeful for the preſent purpoſe. Now, as the ſegments are in the inverſe ratio of the numbers of this table, putting *g* for the number correſponding to the elevation of 15°, and *h* for the number correſponding to any other elevation, the ſegments muſt be multiplied by $\frac{g}{h}$. 3dly, Theſe ſegments will yet be ſubject to another variation depending on the aperture of the teleſcope. It is certain that a larger

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ſtances de Jupiter au Soleil et à la Terre, au moment de cette époque, et *m*, *n*, les diſtances de Jupiter au Soleil et à la Terre, au moment pour lequel on voudra connaitre le ſegment inviſible. 2. En vertu de la hauteur de Jupiter ſur l'horiſon. Les ſegmens, que j'ai obſervés, ont été tous réduits à la hauteur conſtante de 15°. Mr. Bouguer a donné, dans ſon Optique, une table des degrés de la lumiere des planetes, à differentes hauteurs ſur l'horiſon ; je me ſuis aſſuré, par des obſervations, qu'elle eſt très exacte, et très utile dans la recherche préſente. Les ſegmens ſont en raiſon inverſe des nombres de cette table : ſoit donc pour 15° ce nombre *g*, et pour une hauteur quelconque ſoit ce nombre *h*, il faudra multiplier les ſegmens par $\frac{g}{h}$. 3. En vertu de l'ouverture de la lunette. Il eſt certain qu'une ouverture plus grande donnant

larger aperture giving more light, the inſenſible part of the diſk muſt be ſmaller; and it ſeems demonſtrable by theory, that this inſenſible part, or the inviſible ſegments, muſt be inverſely as the ſquares of the apertures. I reſolved, however, to aſſure myſelf of this by experiment. For this purpoſe, I carried my teleſcope to Mr. Meſſier's obſervatory, who has one of Dollond's teleſcopes, of three feet and a half length, and forty lines aperture. On the 20th of Auguſt 1771, he ſaw the ſecond ſatellite diſappear in his teleſcope, through an aperture of three lines. The ſame ſatellite diſappeared in mine, when the aperture was reduced to the ſame quantity of three lines, and not before. We changed inſtruments, and, repeating the experiment, found the ſame effect. Now, the inſenſible part was $\frac{9}{1600}$ of the diſc, in Mr. Meſſier's inſtrument, and $\frac{9}{576}$ in mine. Theſe portions, therefore, in theſe teleſcopes, were in the inverſe proportion of the ſquares of the apertures.

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donnant plus de lumiere, la partie inſenſible du diſque doit être plus petite, et il parait démontré, par la theorie, que, dans différentes lunettes, cette partie inſenſible, ou les ſegmens inviſibles doivent être en raiſon inverſe des quarrés des ouvertures; mais j'ai voulu m'en aſſurer par l'expérience. Je me ſuis tranſporté chez Mr. Meſſier avec ma lunette; il en a une de Mr. Dollond, de trois pieds et demi, qui porte 40 lignes d'ouverture. Le 20 Août 1771, il a vû diſparaitre, dans ſa lunette, le ſecond ſatellite, par une ouverture de 3 lignes. Le même ſatellite n'a diſparu, dans la mienne, que par une ouverture également de 3 lignes. Nous avons repété la même expérience, en troquant d'inſtrumens, et nous avons trouvé les mêmes réſultats. Or, dans la lunette de Mr. Meſſier, la portion inſenſible était $\frac{9}{1600}^{me}$ du diſque, et dans la mienne $\frac{9}{576}$. Ces portions, dans ces lunettes, étaient donc en raiſon inverſe des quarrés des ouvertures.

apertures. Conſequently, in order to determine the ſegments for an aperture of any number of lines k, the ſegments of my table, which are all calculated for an aperture of 24 lines, muſt be multiplyed by $\frac{576}{k^2}$. Hence, to compute the inviſible ſegment for any particular eclipſe, the actual diſtance of the ſatellite from the edge of Jupiter being known, look for the quantity of the inviſible ſegment, which anſwers to that diſtance, in my table, and multiply this quantity by $\frac{m^2 n^2}{p^2 q^2} \times \frac{g}{b} \times \frac{576}{k^2}$. If two different obſervations made in the ſame place, or rather two obſervations, made in different places by different obſervers, are to be compared, the inviſible ſegment muſt be determined, ſuch as it was for each obſerver; A B and A b, the verſed ſines of theſe ſegments,

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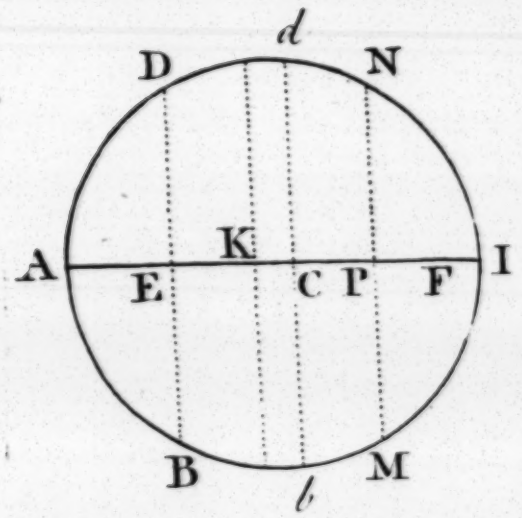
ainſi, mes ſegmens ayant é é déterminés pour une ouverture de 24 lignes, ſi on veut les avoir, pour une lunette, dont l'ouverture aura un nombre de lignes exprimé par k, il faudra multiplier ces ſegmens par $\frac{576}{k^2}$. Donc, quand on voudra calculer le ſegment inviſible, qui aura lieu pour le moment d'une certaine éclipſe, au moïen de la diſtance actuelle du ſatellite au bord de Jupiter, on cherchera, dans mes tables, le ſegment inviſible, qui y répond, et on le multipliera par $\frac{m^2 n^2}{p^2 q^2} \times \frac{g}{b} \times \frac{576}{k^2}$. Si on veut comparer enſemble deux obſervations différentes, ou plûtôt, ſi on veut comparer deux obſervations faites en différens lieux par deux obſervateurs différens, on déterminera le ſegment inviſible, qui a lieu pour chacun de ces obſervateurs: on calculera les ſinus verſes A b et A B de ces deux ſegmens, et dans l'expreſſion $\frac{2Rr(a-b)}{d}$

ſegments, muſt be computed, and in the expreſſion $\frac{2Rr(a-b)}{d}$, the only remaining unknown quantity, will be r. The following is the method I have hit upon for determining it. I conſidered, that, by trying different diaphragms ſucceſſively, ſome few minutes before an immerſion, it would be eaſy to find out the particular ſize, which would make the ſatellite diſappear; and that the proportion of the inviſible ſegment to the whole diſc of the ſatellite, for that inſtant, would, by that means, be determined. Suppoſe then that I have found this diaphragm: my next ſtep is, to cover the object-glaſs of my teleſcope with a diaphragm ſomewhat larger, which ſuffers me juſt to perceive the ſatellite, but ſo weak and ſmall, that the leaſt further diminution of its light muſt render it inviſible. I wait till it actually diſappears; I write down this time, then take away the diaphragm, and the number of ſeconds, which paſs betwixt this firſt diſappearance and the

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$\frac{2Rr(a-b)}{d}$, on n'aura plus d'autre inconnue que r. Voici la méthode que j'ai imaginée pour la déterminer. J'ai penſé, que ſi quelques minutes avant une immerſion, on eſſaïait differens diaphragmes, juſqu'à ce qu'on ait trouvé celui, qui fait diſparaitre le ſatellite, on aurait, pour ce moment, le rapport du ſegment inviſible au diſque entier du ſatellite. Ce diaphragme étant connu, je couvre enſuite l'objectif de ma lunette d'un diaphragme un peu plus grand, qui me laiſſe appercevoir le ſatellite, mais faible et très petit, de maniere que ce ſatellite ceſſera d'être viſible dès que ſa lumiere ſera tant ſoit peu diminuée. J'attends qu'il diſparaiſſe, [*et je ſuis ainſi averti du moment où il commence à toucher l'ombre*] (*b*) je marque cet inſtant, j'enleve le diaphragme, et le nombre de ſecondes écoulé, entre cette premiere diſparition

the true immerſion, giving me a great part of the diameter, I eaſily compute the whole. The following is an example of my method. On the 26th of June 1771, there was an immerſion of the third ſatellite, at 56′ after nine in the evening. I found the diaphragm, which made the ſatellite diſappear, to be of 12 lines. Then I fitted my glaſs with a diaphragm of 17 lines; I might have taken one much ſmaller: preſently the ſatellite diſappears. But removing the diaphragm, I ſee the ſatellite again, very diſtinctly, for 2′ 18″; after which, the true immerſion followed. Now, this is my calculation. The aperture of the diaphragm, which made the ſatellite diſappear, being 12 lines by obſervation, the inviſible ſegment at the inſtant of the eclipſe, muſt have been a quarter of the diſc. Let A B D be this quarter. I know that, at the inſtant of the immerſion, the ſatellite had entered the ſhade, by the whole part E F of its diameter. I ſay then, if on an aperture of 24 lines, the part A B D is inſenſible, the inſenſible part, on an aper-



et la véritable immerſion, me donne la meſure d'une grande partie du diametre, d'où il eſt aiſé de conclure le diametre entier. Voici un exemple de la méthode. Le 26 Juin, 1771, à 9 h. 56′ du ſoir, il y avait une immerſion du troiſieme ſatellite. J'ai déterminé de 12 lignes le diaphragme qui faiſait diſparaitre le ſatellite. J'ai garni ma lunette d'un diaphragme de 17 lignes (je l'aurais pû prendre beaucoup moins ouvert) j'ai vû diſparaitre le ſatellite, j'ai ôté le diaphragme, j'ai revû, très diſtinctement, le ſatellite pendant 2′ 18″, après quoi s'eſt faite la véritable immerſion. Voici le calcul. J'apprends, par l'obſervation du diaphragme

aperture of 17 lines, will be larger than A B D, in the proportion of the ſquare of 24 to the ſquare of 17. Saying, then as $17^2 : 24^2 :: 0,25000 :: x$, x comes out $= 0,49827$, or near half the diſc repreſented by unity; thence I ſee that, at the inſtant of the firſt diſappearance, the ſatellite had not gone in farther than K. Putting the radius AC=1, the verſed ſines AE, AK will be=0,59602 and 0,99884; conſequently, EK = 0,40282. Subſtitute this value of E K, inſtead of $a - b$, in the expreſſion $\frac{2R \times r(a-b)}{d}$, and you will have $\frac{2Rr \times 0,40282}{d}$ $= 2' 18''$, and $r = 5'23''$ of time, will be the ſemi-diameter of the ſatellite, or, the whole diameter will be 22′ 34″ [of the ſatellite's orbit, conſidered as a circle, or, would be ſeen, under an angle of this quantity, from Jupiter's centre]. In this obſervation, as I have already

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phragme de 12 lignes, qu'au moment de l'éclipſe le ſegment inviſible a dû être le quart du diſque : ſoit A B D le quart du diſque ; je ſuis ſûr, qu'au moment de l'immerſion, le ſatellite était entré dans l'ombre, de toute la partie E F de ſon diametre. Je dis enſuite, ſi pour une ouverture de 24 lignes la partie A B D eſt inſenſible, cette partie doit devenir plus grande pour une ouverture de 17 lignes ; et elle doit augmenter dans le rapport du quarré de 17 au quarré de 24 : faiſant donc comme $17^2 : 24^2 :: 0,25000 : x$ on trouve $x = 0,49827$, ou près de la moitié du diſque, repreſenté par l'unité. Je vois donc, qu'au moment de la premiere diſparition le ſatellite n'était entré encore que juſqu'en K. En ſuppoſant le raïon A C = 1, on trouve les ſinus verſes A E, A K égaux à 0,59602, et 0,99884 : par conſequent, E K=0,40282. Si l'on ſubſtituë, pour $a - b$, cette valeur de E K, dans la formule $\frac{2Rr\ (a-b)}{d}$, on aura $\frac{2Rr \times 0,40282}{d} = 2' 18''$: d'où l'on tire $r = 5' 23''$ de tems, pour le demi diametre du ſatellite, ou 22′ 34″ de degrés, pour le diametre entier. Dans cette obſervation, comme je l'ai déja remarqué,

ready ſaid, I uſed a diaphragm with too great an opening (*c*), for the firſt diſappearance. Take then a ſecond obſervation. On the firſt of Auguſt 1771, there was an emerſion of the third ſatellite, about 15′ after nine. I marked the inſtant of this emerſion; then I furniſhed my teleſcope with a diaphragm of 8 lines. The ſatellite diſappeared, and did not begin to appear again till at the end of 6′ 24″. Some minutes after, when it was quite come out of the ſhadow, I meaſured the diaphragm, which would make it diſappear, and found it of 7 lines. Theſe 7 lines give a ſegment A B D of 0,08507. Then ſaying $8^2 : 24^2 :: 0{,}08507 : x$; x, or the ſegment A N M, comes out = 0,76562, A E = 0,27994, A P = 1,43098, and E P = 1,15104. Therefore $\frac{2Rr \times 1{,}15104}{d} = 6' 24''$. From this equation r comes out, 5′ 20″, and the whole diameter 22′ 22″, [in parts of the circular orbit]. Theſe two concluſions agree ſo

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marqué, j'avais emploïé un diaphragme d'une trop grande ouverture, pour la premiere diſparition. Voici une ſeconde obſervation: Le 1 Août 1771, à 9 h. 15′, il y avait une émerſion du troiſieme. Je marquai l'inſtant de cette émerſion, enſuite je garnis ma lunette d'un diaphragme de 8 lignes: le ſatellite diſparut, et il ne commença à reparaître qu'au bout de 6′ 24″; quelques minutes après, étant tout à fait ſorti de l'ombre, je meſurai le diaphragme qui le faiſait diſparaitre, il ſe trouva de 7 lignes. 7 lignes donnent un ſegment A B D de 0,08507. Faiſant enſuite $8^2 : 24^2 :: 0{,}08507 : x$, on trouve x ou le ſegment A N M = 0,76562: on trouve A E = 0,27994, A P = 1,43098, et E P = 1,15104. On a donc la formule $\frac{2Rr \times 1{,}15104}{d}$ = 6′ 24″: d'où l'on tire r = 5′ 20″, et lediametre entier, en degrés de 22′ 22″. Ces deux reſultats s'accordent merveilleuſement bien,

ſo perfectly well with each other, that, if I am not too fond of my own work, I may venture to ſay, the method, I have invented, may be carried to great exactneſs. I hope you will have the goodneſs to give me your opinion of it, which I have the greateſt reſpect for, and will be very uſeful to me, eſpecially if you have leiſure to repeat the obſervations. I ſhall take the liberty to ſubjoin a few hints, upon the manner of making them, at the end of this letter. The diameter of the firſt ſatellite I have determined by three obſervations as follows:

An	Immerſion	1771, June 30,	gave	7′ 17″	in time or	1° 1′ 45″ as ſeen
	Emerſion	Aug. 1,		7′ 3″		0° 59′ 46″ from
	Emerſion	Sept. 2,		7′ 1″		0° 59′ 29″ ☿.

You ſee, Sir, that this agreement is likewiſe very ſatisfactory. Mr. Meſſier took part in theſe obſervations, and found the application of them very eaſy. He himſelf

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bien, et ſi je ne m'abuſe pas trop ſur mon travail, il me ſemble, que la méthode, que je viens de vous expoſer, eſt ſuſceptible d'une très grande exactitude. J'eſpere que vous voudrez bien m'en dire votre ſentiment, dont je fais le plus grand cas, et qui me ſera très utile, ſurtout ſi vous avez le loiſir de répéter ces obſervations. Je prendrai la liberté d'ajouter, à la fin de cette lettre, quelques réflexions ſur la maniere de les faire. A l'égard du diametre du premier, je l'ai déterminé par trois obſervations. L'immerſion du 30 Juin 1771, m'a donné ſon diametre de 7′ 17″ en tems, ou de 1° 1′ 45″ vü de Jupiter. Le premier Août l'émerſion m'a donné ſon diametre de 7′ 3″ en tems, ou de 0° 59′ 46″; enfin, l'émerſion du deux Septembre m'a donné ſon diametre de 7′ 1″ en tems, ou de 0° 59′ 29″. Vous voïez, Monſieur, que cet accord eſt encore très ſatisfaiſant. Mr. Meſſier a bien voulu prendre part à ces obſervations, il en a trouvé l'uſage facile.

ſelf obſerved the diameter of the ſecond ſatellite, by the emerſion of the 30th of Auguſt; but it emerged at ſo ſmall a diſtance from the firſt, that this circumſtance may have vitiated the obſervation; the diameter of this ſatellite muſt therefore be verifyed by freſh obſervations. However, the reſult of Mr. Meſſier's makes it 7′ 2″ in time, or 29′ 42″ ſeen from Jupiter; and a former obſervation of mine, of the 11th of July, gave the ſame quantity preciſely. Thus, by means of the tables given in my paper, which I had the honour of mentioning to you, it will be poſſible, to compute the inviſible ſegment, for all the obſervations which have been hitherto made, and, the diameter of the ſatellite being likewiſe aſcertained, to reduce the inſtant of the obſerved eclipſe to that of the paſſage of the center over the edge of the ſhadow, which will be a fixed term for all the obſervations, and

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Il a obſervé lui même le diametre du ſecond, par l'émerſion du 30 Août, mais il ſortit ſi près du premier, que cette circonſtance peut avoir nui à la bonté de l'obſervation; ainſi le diametre de ce ſatellite a beſoin d'être confirmé par de nouvelles obſervations. Quoiqu'il en ſoit, il réſulte de ſon obſervation, que le diametre du ſecond eſt de 7′ 2″ en tems, ou de 29′ 42″ vû de Jupiter. Je l'avais trouvé par une autre obſervation du 11 Juillet, de 7′ 2″, préciſement comme Mr. Meſſier. Au moïen des tables, que j'ai données dans mon mémoire, et dont j'ai eu l'honneur de vous parler, Monſieur, on pourra donc calculer, pour toutes les obſervations faites juſqu'à préſent, le ſegment inviſible, qui a eu lieu, et, au moïen du diametre connu réduire l'inſtant de l'éclipſe, au moment où le centre s'eſt trouvé ſur le bord de l'ombre, moment qui ſera un terme fixe pour toutes les obſervations, et pour tous les obſervateurs,

and all the obſervers, who but ſeldom agree in the obſervation of the ſame eclipſe. I confeſs, that the tranſparency of the air is not always the ſame, and that a greater or leſs degree of tranſparency will make the ſegments ſmaller or larger, and conſequently affect the obſervation. The inequality of ſight may likewiſe occaſion ſome error; for, though it might be poſſible, to ſettle the general effect of the difference of ſight of different obſervers, the ſight of the ſame perſon is not conſtantly the ſame, and even independently of the change produced by age, may not have the ſame ſtrength at all times. But by the method I propoſe, all theſe inconveniencies will be remedied, in future obſervations, with little trouble. Every obſerver is to furniſh himſelf with ſeveral diaphragms of paſteboard, gradually diminiſhing by half-lines, to be applied to the object-glaſs externally, and ſome minutes before an immerſion, or after an emerſion, he is to determine which of them intercepts

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teurs, qui ne s'accordent que rarement dans l'obſervation de la même éclipſe. Je conviens, que la tranſparence de l'air n'eſt pas toujours la même, et que quelques degrés de plus ou de moins dans cette tranſparence rendent les ſegmens moins ou plus grands, et conſéquemment affectent l'obſervation. L'inégalité des vuës eſt encore une autre ſource d'erreurs; quand même on pourrait établir l'effet de la difference des vuës des obſervateurs; ces vuës ne ſont pas conſtantes, et, ſans parler du changement, que l'age amene avec lui, la vuë peut n'avoir pas la même force dans tous les inſtans. Voici la méthode, que je propoſe, pour remedier à tous ces inconveniens, d'une maniere facile et commode, à l'égard des obſervations futures. Il ſuffirait, que chaque obſervateur fit des diaphragmes de carton, pour appliquer exterieurement à ſon objectif, qui diminuaſſent de demi ligne en demi ligne. Quelques minutes avant une immerſion, ou après une émerſion, il

intercepts from him the ſight of the ſatellite. Having found this, and knowing likewiſe the diameter of the ſatellite, he will reduce [by the proceſs of calculation already explained] the obſerved inſtant of the eclipſe, to that of the paſſage of the center; which is the ſame [as I ſaid before] for all the obſervers in the world. You ſee, Sir, what advantages would ariſe, from this agreement, for the theory of the ſatellites, and the preciſion of the terreſtrial longitudes. This method takes in every thing; the difference of glaſſes, that of ſights, the greater or leſs tranſparency of the atmoſphere, &c. Obſervation gives the ſegment greater or leſs, in proportion to the combined influence of all theſe cauſes. The principal advantage of this method, which requires only a very ſimple calculation, is, that it depends on no hypotheſis. It enables us to meaſure immediately the light of the ſatellite, whether increaſed or diminiſhed by all the cauſes above-mentioned; to meaſure, I ſay, the real impreſſion of that

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il déterminerait, lequel de ces diaphragmes lui dérobe la vuë du ſatellite: enſuite, au moyen du diametre, il reduirait l'inſtant de l'éclipſe obſervée, au moment du paſſage du centre, moment qui eſt le même pour tous les obſervateurs de tous les pays du monde. Vous voyez, Monſieur, quels avantages il réſulterait de cet accord, pour la theorie des ſatellites, et pour la préciſion des longitudes terreſtres. Cette méthode renferme tout; la différence des lunettes, celle des vuës, le plus ou moins de tranſparence de l'atmoſphere, &c. C'eſt en raiſon de toutes ces cauſes, que l'on trouve, par obſervation, le ſegment plus ou moins grand. L'avantage principal de cette méthode, qui n'exige qu'un calcul fort ſimple, eſt, de n'être ſoumiſe à aucune hipotheſe. On meſure immédiatement la lumiere du ſatellite, augmentée ou diminuee par

that light upon the eye, whatever be the actual ſtate of the organ. Theſe, Sir, are the methods and enquiries which I ſubmit to your judgment, and with that view, I have been very copious in my details. I am highly ambitious of your approbation, and ſhall pay great attention to your remarks. I muſt add, that I am ſenſible, the determination of the inviſible ſegment, by means of the diaphragm, might be inconvenient to thoſe, who make uſe of large teleſcopes for the eclipſes of the ſatellites, were it not, that this obſervation may be equally well made with a ſmaller teleſcope, provided only, that it be ſufficicent to ſee and diſtinguiſh the four ſatellites; and after the diaphragm is determined by this ſmaller teleſcope, the larger one may be uſed for the obſervation of the eclipſe. For theſe meaſures are eaſily transferred from one inſtrument to another, the inviſible ſegments in different teleſcopes, being inverſely as

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par toutes les cauſes que nous avons indiquées, et on meſure même l'impreſſion de la lumiere ſur l'œil, quel que ſoit l'état actuel de cet organe. Voila, Monſieur, les méthodes et les recherches, que je ſoumets à vos lumieres; et je l'ai fait avec beaucoup de détail, afin de vous mettre dans le cas de les juger. J'ambitionne extremement votre ſuffrage, et je ferai uſage de vos reflexions. J'ajouterai, que la détermination du ſegment inviſible, par le moyen du diaphragme, pourrait devenir incommode, pour ceux qui ſe ſervent de grandes lunettes, dans les éclipſes des ſatellites ; mais cette obſervation peut ſe faire également avec une petite lunette, pourvû qu'elle faſſe bien voir et diſtinguer les quatre ſatellites. Quand on aura déterminé le diaphragme avec cette lunettte, on fera l'obſervation de l'éclipſe avec [illegible] ordinaire. On peut tranſporter ces meſures, [illegible] d'une à l'autre, puiſque les ſegmens

as the ſquares of the apertures. For reflectors, I have a method of the ſame kind with the former, grounded, at leaſt, upon the ſame principles, by which I can determine their power, and compare them, both with each other, and with the refracting teleſcopes. I ſhall conclude with ſome hints concerning the obſervation of the diaphragm, for determining the inviſible ſegment. To repeat theſe obſervations with judgement, it will be neceſſary to recollect the intention of them; which is, to meaſure what portion of the diſc remains illumined, that is, what portion of the ſatellite's light [continues, though unperceived, to be tranſmitted to the obſervers eye] at the inſtant when the ſatellite diſappears, upon the brink of an eclipſe. In leſſening gradually the aperture of the glaſs, the obſerver ſhould not begin with too ſmall an opening; becauſe the eye, not accuſtomed

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mens inviſibles ſont, dans les differentes lunettes, en raiſon inverſe du quarré des ouvertures. Quant aux téleſcopes de reflexion, j'ai une méthode à peu près du même genre que celle-ci, et qui du moins eſt fondée ſur le même principe, par laquelle je puis déterminer la force des teleſcopes, et les comparer, tant entr'eux, qu'avec les lunettes. Je finirai, Monſieur, par quelques réflexions ſur l'obſervation du diaphragme, qui détermine le ſegment inviſible. Pour bien répéter ces obſervations, il faut ſe rapeller l'eſprit, dans lequel elles ont été entrepriſes, et dans lequel elles doivent toujours être faites. C'eſt de meſurer la quantité de lumiere, la portion éclairée du diſque, qui ſubſiſte encore au moment où le ſatellite diſparait, en s'éclipſant. Quand on diminuë, par degrés, l'ouverture de la lunette, il ne faut pas commencer par une trop petite ouverture: l'œil n'eſt pas fait à la grande obſcurité qui en réſulte; on ne verrait point le ſatellite. A chaque

fois

to the great obſcurity which follows, might not ſee the ſatellite at all. As the opening is gradually contracted, the ſatellite ſeems to grow leſs. The obſerver ſometimes loſes ſight of it for a moment; but, if he continues to look attentively, he ſees it again. The real diſappearance is only to be concluded, when, upon fixing with ſteady eyes, for about half a minute, on the place it occupied, it is ſeen no more; for if one perſiſted to obſerve it much longer, it might happen, that it might be ſeen to glimmer at times, and immediately diſappear. I have always made it a rule, to conſider the debilitation of the light, in this degree, as actual diſparition, and it is neceſſary, that obſervers ſhould agree upon this point, in order that their different eſtimations may be conſiſtent. Theſe fits, of momentary glimmering and extinction, are undoubtedly owing to the motion of the particles of the atmoſphere. In the cleareſt weather, there are always particles of vapour floating in it, in vaſt abundance; according as theſe particles place themſelves

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fois qu'on diminuë l'ouverture, on voit diminuer le ſatellite: quelquefois on croit ne le plus voir, mais, après y avoir fait attention, on le revoit: il faut le juger diſparu, quand on ne l'apperçoit plus, après avoir conſidéré, avec des yeux bien repoſés, pendant une demie minute environ, la place qu'il occupe: car, ſi on s'attachait à le conſidérer plus longtems, il pourrait arriver, qu'on le revit, par moment, paraitre, comme par éclairs, & diſparaitre auſſitôt. Je l'ai toujours jugé diſparu, quand il a été réduit à cet état d'affaibliſſement; il eſt eſſentiel de convenir de ceci, afin que les meſures ſe rapportent. Ces variations ſont duës, ſans doute, au mouvement de l'atmoſphere. Il y a toujours, dans les momens

ſelves in the direction of the ray of light, or out of it, the light of the ſatellite is diminiſhed or reſtored, and the ſatellite, in conſequence, is either hid or rendered viſible. This does not happen in eclipſes, wherein a great part of the light is in reality extinguiſhed. But, in the caſe I am now ſpeaking of, though the diminution of the aperture of the glaſs does indeed take away a great quantity of it, yet this quantity is always relative to the actual ſtate of the atmoſphere (*d*): if that ſtate changes, this quantity becomes alternately ſenſible or inſenſible, according as the light meets with more or leſs obſtructions, in its paſſage, from the vapours. Another thing, which it will be neceſſary to point out to you, is, that the operation with the diaphragms, for determining the inviſible ſegment, muſt be made and concluded, before the ſatellite has touched the ſhadow. The proper time, therefore

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momens où le tems eſt le plus ſerein, une infinité de vapeurs, dont les particules nagent dans l'atmoſphere; ſelon que ces particules s'accumulent ſur la route du raïon de lumiere, ou qu'elles s'en écartent, elles ôtent ou rendent plus de lumiere à ce ſatellite, qui, en conſéquence, ſe cache ou ſe laiſſe voir. C'eſt ce qui n'arrive point dans les éclipſes, où une grande partie de la lumiere eſt réellement détruite. Ici l'ouverture diminuée de la lunette en détruit réellement auſſi une grande quantité, mais cette quantité eſt toujours rèlative à l'etat actuel de l'atmoſphere. Si l'état de l'atmoſphere change, cette quantité devient alternativement ſenſible ou inſenſible, ſelon que les vapeurs lui oppoſent plus ou moins d'obſtacles. Une choſe qu'il eſt encore néceſſaire de vous faire obſerver, Monſieur, c'eſt, que l'operation du diaphragme, qui détermine le ſegment inviſible, doit être faite et finie, avant que le

fore, for beginning this obſervation, will be determined, by the time the diameter takes in entering, which, in the perpendicular ingreſs, or when Jupiter is in the nodes, is 7′ for the two firſt, and 11′ for the third. The time of the oblique ingreſs is (7′) $\frac{2R}{d}$ for the two firſt, and (11′) $\frac{2R}{d}$ for the third; which, for this laſt, may in extreme caſes amount to about 27′ or 28′. It is proper to take 5′ more; for the obſervations of the diaphragm will take up 2′, even when uſe has rendered them familiar, and the tables may be 2′ or 3′ behind. At preſent, it is be ſufficient, to begin the obſervation 16′ before an eclipſe of the third ſatellite; but there are times, in which it would be neceſſary, to begin 29′ or 30′ before. It is eſſential, not to begin too late, for fear of miſſing the obſervation; it is likewiſe eſſential, not to begin too ſoon, becauſe then the ſegment

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le ſatellite ait touché l'ombre. Ainſi le tems, que le diametre met à entrer, doit donc regler cette obſervation; c'eſt à dire, 7′ pour les deux premiers, et 11′ pour le troiſième; mais ce tems eſt celui de l'entrée perpendiculaire, qui n'a lieu que lorſque Jupiter eſt dans les nœuds. L'entrée oblique eſt 7′ $\frac{(2R)}{d}$ pour les deux premiers, et (11′) $\frac{2R}{d}$ pour le troiſième, ce qui peut aller pour celui ci juſqu'a 27′ ou 28′. Il faut encore prendre 5′ de plus, parceque même étant exercé, il faut bien 2′ pour l'obſervation du diaphragme, et que les tables peuvent être en retard de 2′ à 3′. Actuellement il ſuffit donc, de s'y prendre, pour cette obſervation 16′ avant une eclipſe du troiſième, mais il y a des tems, où il faudroit s'y prendre 29′ ou 30′ avant. Il

ſegment meaſured would be too ſmall, as the ſatellite is continually either approaching Jupiter, or receding from him. All this is haſtily explained; but theſe matters are ſo familiar to you, that you cannot but underſtand me, and this letter is already too long. I am afraid it will tire you; but I am extremely deſirous of having the exactneſs and utility of theſe two methods, the one, for the meaſure of the diameters, the other, for making all the obſervations capable of mutual compariſon, aſcertained, by repeating the obſervation of the diaphragm in every eclipſe. I cannot take a better way, than to conſult the ſeveral aſtronomers, who, like you, beſides being deeply ſkilled in the theory, are the moſt celebrated obſervers. If they will adopt this method, it will be the beſt way of making it general, as others will follow it of courſe. I have communicated

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eſt eſſentiel de ne s'y pas prendre trop tard, pour ne pas manquer l'obſervation; il eſt eſſentiel auſſi de ne s'y pas, prendre trop tôt, parcequ' alors on meſurerait un ſegment trop petit; car le ſatellite s'approche ou s'éloigne continuellement de Jupiter. Tout ceci eſt expliqué en courant, comme vous voyez, Monſieur, mais vous en ſavez trop ſur ces matieres, pour ne me pas entendre, et cette lettre eſt déja d'une exceſſive longueur. J'ai bien peur, qu'elle ne vous cauſe de l'ennui; mais j'ai extrememente à cœur, de conſtater l'exactitude et l'utilité de ces deux méthodes, celle de la meſure des diametres, et celle de rendre toutes les obſervations comparables, en répétant, à chaque éclipſe, l'obſervation du diaphragme. Je ne puis mieux faire que de conſulter les aſtronomes, qui étant comme vous, Monſieur, profondement verſés dans la theorie, ſont, en même tems, les plus célébres obſervateurs. Si ceux ci veulent bien adopter cette méthode, c'eſt le meilleur moyen de la rendre générale: les autres l'adopteront auſſi.

municated it to Mr. Meſſier, who propoſes making uſe of it. Mr. Maraldi, who is gone to his houſe near Nice, has tried the obſervation of the diaphragm, with ſucceſs, with an achromatic teleſcope three feet and a half long; and he would already have made uſe of it, but that it is impracticable with the teleſcope of 15 feet, which he uſes for the eclipſes of the ſatellites. I have written to him, that he may obſerve the eclipſe with his uſual teleſcope, and the diaphragm with the achromatic; ſo that I make do doubt he will uſe this method, as ſoon as Jupiter ſhall have come out of the rays of the Sun. Theſe, Sir, are the things on which I wiſh to conſult you, and have your advice. I ſhall be much

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Je l'ai communiquée à Mr. Meſſier, qui ſe propoſe d'en faire uſage. Mr. Maraldi, qui, comme vous ſavez, eſt allé chez lui à Perinaldo, dans le comté de Nice, a eſſaïé l'obſervation du diaphragme, qui lui a réüſſi, avec une lunette achromatique de 3 pieds et demi; et il l'auroit déja emploïée, ſi ce n'eſt, que l'obſervation du diaphragme n'eſt pas practicable avec ſa lunette de 15 pieds, dont il ſe ſert pour les éclipſes des ſatellites. Je lui ai mandé, comme je vous l'ai fait obſerver plus haut, qu'il pouvoit faire l'obſervation du diaphragme avec ſa lunette achromatique, et celle de l'éclipſe avec ſa lunette ordinaire: ainſi je ne doute point, qu'il ne faſſe uſage de cette méthode, lorſque Jupiter ſe dégagera des raïons du ſoleil. Voila, Monſieur, toutes les choſes, ſur leſquelles je ſuis bien aiſe de vous conſulter, et de vous demander votre avis. Si vous croyez que cette lettre en vaille la peine, je ſerai très flatté, que vous la communiquiez à la Societé Roïale, à laquelle je préſente l'hommage de mon profond reſpect.

much flattered by your communicating this letter to the Royal Society, if you think it deſerves attention.

I have the honour to be, &c.

Bailly,

Of the Royal Academy of Sciences of Paris.

Pardon, Monſieur, de vous avoir détourné ſi longtems. Permettez moi de vous aſſurer du reſpect avec lequel je ſuis,

Votre très humble

Et très obeïſſant Serviteur,

BAILLY,

De l'Academie des Sciences de Paris.

NOTES

NOTES ON THE FOREGOING PAPER,

BY THE REV. SAMUEL HORSLEY.

(a) $\frac{2Rr(a-b)}{d}$.

This formula is deduced from the following principles.

1ſt. That the motion of the ſatellite, in its orbit, is uniform, or, at leaſt, may be conſidered as ſuch, without ſenſible error, in the preſent inveſtigation.

2. That the time, which the ſemidiameter takes to enter the ſhadow, in any eclipſe, is inverſely as the whole time of the duration of the eclipſe.

3. That the time, which any given part of the ſemidiameter takes to enter the ſhadow, is to the time which the whole ſemidiameter takes to enter, as that part to the whole.

Now, let a and b denote the verſed ſines of the arcs Ad, AD (in the figure p. 189.) reſpectively, the radius being unity. Let R denote the half-time of the duration of an eclipſe, when Jupiter is in the node of the ſatellite's orbit. r, the time which the ſemidiameter takes to enter the ſhadow in *ſuch* eclipſes; d, the whole duration of an eclipſe, happening when Jupiter is at any given diſtance from the node. Then will $\frac{2Rr}{d}$ expreſs the time, which the ſemidiameter of the ſatellite will take to enter the ſhadow, in the eclipſe whoſe duration is d (by 2^d, becauſe $d : 2R = r : \frac{2Rr}{d}$). And, $\frac{2Rr}{d}$ being the time that the ſemidiameter takes to enter the ſhadow, $\frac{2Rr(a-b)}{d}$ will be the time that the part Bb takes to enter, by 3^d."

It is to be obſerved, that, to compare two eclipſes by this formula, it is neceſſary, that the planet ſhould have been at the

ſame diſtance from the node of the ſatellite's orbit, at the commencement of both. For comparing eclipſes otherwiſe circumſtanced, a more general formula may eaſily be deduced from the ſame principles. If A*d* be the inſenſible ſegment in one eclipſe, AD in another (vide figure p. 189.), *a* the verſed ſine of the arc A*d*, *b*, of AD (the radius being unity), *d* the whole duration of the firſt eclipſe, δ of the ſecond, then $\frac{2Rr}{d\delta} \times \overline{\delta a - db}$ is what the author would call the equation, between the two, ariſing from the different magnitudes of the inſenſible ſegment, or the time, by which the interval between the obſerved eclipſes, differs from the interval between the real paſſage of the center in each eclipſe. This is a general formula, for all eclipſes of the ſame ſatellite. If the planet's diſtance from the node hath been the ſame in both, then $\delta = d$, and this formula changes into the author's. The more general one is here given, rather for the fuller explication of the theory, than for any neceſſity that there is to have recourſe to it in practice. For, though the uſe of it may ſometimes be convenient, eclipſes of the ſame ſatellite may always be compared without it, when once the diameter of the ſatellite is known, and the magnitude of the inſenſible ſegment in each eclipſe determined, by reducing the obſerved immerſion or emerſion to the true ingreſs or egreſs of the center.

(*b*) The words printed in the Italic character are deſignedly omitted in the tranſlation, it being apprehended, that it is owing to ſome inadvertency, that they appear in the author's text. For unleſs they are expunged, the general deſcription, here intended, of the author's method of determining the diameters of the ſatellites, will by no means agree with the examples of that method immediately ſubjoined. Theſe words imply, that the author takes the inſtant of the diſparition of the ſatellite, in the contracted aperture of the diaphragm, for the moment of the contact of the ſatellite's limb with the edge of the ſhadow, and makes that moment, ſo determined, the baſis of his calculations: reaſoning, as it ſhould ſeem, thus. "When any part of the diameter of the ſatellite, however ſmall, hath entered the ſhadow, ſome part of the light, which the obſerver receives, through the aperture of the diaphragm, from the whole unſhaded diſc, will be intercepted. But that aperture is ſo ſmall, that the light tranſmitted through it, from the whole unſhaded diſc, is but juſt ſufficient to be ſenſible; and muſt, therefore, ceaſe to be ſo,

when it is in the ſmalleſt degree diminiſhed, i. e. when the very ſmalleſt part imaginable of the diſc is ſhaded. Therefore, the moment of the diſparition, in the aperture of the diaphragm, is the true commencement of the eclipſe, or differs from it by leſs than any aſſignable difference."

But it appears, from the examples given afterwards, that the author's calculations proceed upon much ſafer principles. Having determined the portion of the diſc, that is inſenſible on the whole given aperture of his teleſcope, he computes what larger portion will be inſenſible, on the ſmaller given aperture of his diaphragm. And then, by obſerving the two diſparitions, the earlier one in the diaphragm, the other in the teleſcope with the object-glaſs uncovered, the laſt of which he calls the true immerſion, he knows the time, in which a given portion of the diameter enters the ſhadow, and, conſequently, the time, in which the whole enters; which determines the magnitude of the whole, in parts of the ſatellite's orbit, or its apparent magnitude to an obſerver at Jupiter's center.

(c) The diſadvantage of uſing too great an aperture is, that the part of the diameter obtained by obſervation, from which the whole is to be concluded, will be leſs, than the ſame method of obſervation would give, with a more contracted aperture. For the larger the aperture of the diaphragm is, which is applied to the object-glaſs, the leſs is the difference between that aperture and the whole aperture of the teleſcope; conſequently, the leſs is the difference between the ſegments, which are inſenſible in theſe apertures ſeverally, and the leſs the portion of the diameter, which paſſes over the ſhadow's edge, between the two diſparitions.

(d) In eclipſes, when once the ſatellite hath diſappeared, or is become viſible, the author ſays, we are not to expect thoſe fits of glimmering and extinction, which he hath deſcribed as taking place, when we obſerve the uneclipſed ſatellite with very contracted apertures.

The reaſon is plainly this. In immerſions, a part of the diſc is ſtill indeed enlightened, when the ſatellite diſappears; and the quantity of light, tranſmitted from this part to the obſerver's eye, muſt be very different, in different ſtates of the air's tranſparency; and conſequently, the ſatellite, after having diſappeared, might become viſible again, by a ſudden increaſe of the air's tranſparency in the tract of the ſatellite's light, provided, the

the magnitude of the unſhaded part remained, at the inſtant of the increaſed tranſparency, what it was, when the ſatellite firſt diſappeared. But, as this is not the caſe, as the unſhaded part is continually growing leſs, the ſatellite cannot re-appear, unleſs the increaſe of tranſparency be ſuch, as to overbalance the diminution of light made by the progreſs of the eclipſe. And the motion into the ſhade is ſo quick, that this can rarely, if ever, happen. By the like reaſoning, fits of extinction are not to be expected, when once the ſatellite hath ſhewn itſelf in an emerſion.

The author of theſe remarks doth not imagine, that any apology is neceſſary for the liberty he hath taken. He hath the higheſt opinion of the merit of Mr. Bailly's invention; and this hath excited him, to contribute what he could, to obviate objections, and to prevent miſtakes.

XXVI. *A Letter from Mr.* Bernard, *of* Deptford, *to Mr.* Robertſon, *Lib. R. S. containing a ſhort Account of an Exploſion of Air, in a Coal-Pit, at* Middleton, *near* Leeds *in* Yorkſhire.

SIR,

Read Feb. 25, 1773. I Have at length procured, from my father, a memorandum made by him on the ſpot, of the effects of foul air ſet on fire, which I have copied exactly. If it affords you the leaſt ſatisfaction, I ſhall be well pleaſed, though late, in having fulfilled my promiſe.

I am, with great reſpect,

Yours,

Grove-Street, Deptford,
January 12, 1773.

W. Barnard.

MEMO-

MEMORANDUM.

BEING engaged in Middleton wood, the eſtate of —— Brandling, Eſq; near Leeds in Yorkſhire, in directing the falling and barking of a large quantity of timber bought of him in May 1758, I was witneſs of the following accident. Some miners, being to renew their operations on the ſhaft of a coal-pit, which, in a former year, had been ſunk to the depth of ſixty yards, in order to get through a ſtratum of very hard ſtone, thought proper to drill holes, and fill them with gunpowder. They afterwards, from the top, threw down fire to blaſt the ſtone, which made a report little louder than that of a piſtol; but the blaze, ſetting the foul air on fire, produced an effect truly ſhocking. The whole wood was ſhaken, the works at the mouth of the pit were all blown to pieces, and the exploſion was ſuch as cannot be deſcribed. The vacuum in the air was ſo conſiderable, that oak trees of a load or more each, at a great diſtance from the pit's mouth, that before ſtood upright, ſtooped towards the pit very much, and muſt have fallen wholly down, had not the air been inſtantly replaced. The bark-pullers, at a quarter of a mile from the pit, were ſo alarmed by the ſhaking and exploſion, that not one of them would have remained in the wood, had they attempted to blaſt it again.

N. B. The trees in the whole circuit ſtooped towards the pit.

THE END OF VOLUME LXIII. PART I.

PHILOSOPHICAL

TRANSACTIONS.

VOL. LXIII. PART II.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

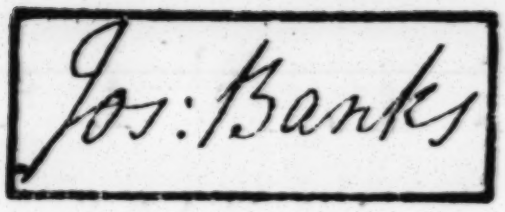
Considerable Parts of the WORLD.

VOL. LXIII. PART II.

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Jos: Banks

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XXVII. *Extract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon *in* Rutland, 1772, *by* T. Barker, *Eſq; Communicated by Sir* John Pringle, *Bart. P. R. S.*

Read March 4, 1773.

		Barometer.			Thermometer.						Rain.
					In the houſe.			Abroad.			
		Higheſt.	Loweſt.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
Jan.	Morn.	29.94	28.35	29.26	44	31	36	45	10	30	2.145
	Aftern.				45	30½	37	51	25	35½	
Feb.	Morn.	29.65	28.65	29.11	46	29	37	46	13	31	3.477
	Aftern.				45½	30½	38	52	24	38½	
Mar.	Morn.	29.64	28.61	29.20	47½	33	41	47	23	34	2.346
	Aftern.				49	34½	42	57	32	43½	
April	Morn.	29.90	28.97	29.50	51	37½	44½	50	27	39	0.882
	Aftern.				52	40	46	57	35	48	
May	Morn.	30.02	29.07	29.66	55	45½	50½	55	33½	45	1.869
	Aftern.				56	46	52	68	47	55½	
June	Morn.	29.99	29.07	29.66	68	51½	59½	66	47	56	3.890
	Aftern.				70	53½	61	81	58½	68	
July	Morn.	29.99	28.96	29.61	65	59½	62	64	48½	56	0.891
	Aftern.				66	60½	63	73	62	68	
Aug.	Morn.	29.91	28.95	29.51	67	56½	61½	61	44	54½	1.678
	Aftern.				68	60	63	76	61	67½	
Sept.	Morn.	29.84	28.57	29.42	62½	52½	58	61½	40	51	4.515
	Aftern.				65	54	59	71	54½	60½	
Oct.	Morn.	29.95	28.77	29.52	58	51½	55	57	39	48	3.267
	Aftern.				59	52	56	64½	47½	57½	
Nov.	Morn.	29.93	28.50	29.26	55½	41	47	56	31½	41	2.461
	Aftern.				56	42	48	59	39½	46½	
Dec.	Morn.	30.06	28.78	29.57	48½	36	42½	50	24	38	1.226
	Aftern.				49½	36½	43	51½	28½	40½	
											28.647

January began mild, but ſoon inclined to froſt; and about the middle of the month, a ſevere ſeaſon ſet in; much froſt, and great ſnows, which would have been very great indeed, if it had been all lying together; but they were intermixed with thaw, rain, and floods, and continued to the middle of March. This cauſed a great expence of hay, and, with the very backward ſpring, and frequent cold weather, even to the middle of May, made few people have any conſiderable quantity of hay left. There were, however, ſome intervals of mild weather, and more graſs this ſpring than laſt.

The ſummer was, in all the ſouth of England, very dry, and burning. There was ſo little graſs about London, that many were forced to fodder their cattle, even in the height of it; but in all the middle of England, the ſummer was a very fine one; no cold weather, nor in general very hot, but chiefly very fair and fine, and a ſufficient quantity of rain came, whenever we wanted it: ſo great a quantity of hay, ſo well got, was hardly ever known. The beginning of harveſt was alſo well got in, but the latter part of it, for it was a late one, was, in this country and northward, exceeding bad; and in this wet weather, ſome wheat ſuffered, moſt of the barley, and all the beans and peaſe. The crop of wheat, where it was well gotten, was tolerable good, the barley yielded worſe, white peaſe were plentiful enough, but ill got, beans and grey peaſe univerſally a failing crop, much of the ſeed being burſt in the wet ſeed-time. This bad latter part of harveſt was in September, which was all very wet, as it was alſo, in a leſs degree, till the beginning of December,

cember, but ſo warm, that the graſs grew till the middle of October as faſt as in ſummer; but the wheat ſeed-time was bad, which did but indifferently on wet land, though it came up very well on light ſoils. The ſeaſon continued mild and open, the graſs ſpringing, and ſcarce a morning that could be called froſty, till December 22, when a calm and moderate froſt concluded the year.

Wet ſummers make plenty of graſs, but drier ſeaſons are more favourable to corn. The moſt plentiful ten years in my time were from 1741 to 1750, which were alſo the drieſt; and the moſt failing ſeries of crops have been ſince that remarkable wet year 1763; ſince which, there has been, in general, much more rain than before.

P. S. Be pleaſed to correct the following errata, in my former letter, Phil. Tranſ. Vol. LXI. p. 223.

	For	Read
Feb. 10.	E. by W.	E. by N.
16.	E. by W.	E. by N.
Aug. mean 36 to 70.	2.194.	2.184.
Nov. 1737.	9.570.	0.570.

XXVIII. *Obſervations on the Lagopus, or Ptarmigan; in a Letter from the Hon.* Daines Barrington, *V. P. R. S. to* Mathew Maty, *M. D. F. R. S.*

DEAR SIR,

THE many different ſpecimens of Lagopi, both in their winter and ſummer plumage, which have lately been preſented to the Royal Society, from Hudſon's Bay, enable us to correct many miſtakes that have hitherto been made in the deſcription of this bird; as well as the unneceſſarily multiplying the ſpecies of the Tetrao genus.

As that able and ingenious naturaliſt, M. de Buffon, is the laſt ornithologiſt who hath made any obſervations on this bird, it may not be improper to take notice of ſome of his ſuppoſed inaccuracies.

The Lagopus, of which M. de Buffon gives an engraving, is in its winter plumage; and the feet of the bird are conſequently covered very thick with feathers. M. de Buffon, however, from not having examined the ſpecimens of the Lagopus with proper attention, ſays, that Ariſtotle could not have been acquainted with this bird, becauſe the under parts of the claws are entirely covered with feathers; which circumſtance is ſo very ſtriking and peculiar, that it could not have eſcaped this father of natural hiſtory.

If a winter ſpecimen, however, of the Lagopus, or Ptarmigan, is accurately examined, it will be found,

found, that no feathers grow preciſely under the claws; though, by wrapping very thickly round them, they have very ſtrongly that appearance: and, in a ſummer ſpecimen, not only the feet, but even the legs, are rather bare of plumage. If Ariſtotle, therefore, had procured the bird in its ſummer dreſs, he could not have obſerved this very ſtriking circumſtance, which M. de Buffon relies upon as ſo ſtrongly characteriſtic.

The ſame difference between the plumage in ſummer and winter is experienced in each of the three ſpecies of Tetrao, which have (according to one of Linnæus's ſubdiviſions) feathered feet; and it is uſually ſaid with us, that they have in winter their ſnow-boots. M. de Buffon, therefore, unjuſtly charges the author of the Britiſh Zoology for ſuppoſing, that this is a wiſe proviſion of Nature againſt the inclemency of the ſeaſon, when he ſays, [*a*] that the vrogallus Minor, or our Black Cock, hath not the ſame protection for its feet, though it buries itſelf under the ſnow, and, becoming torpid, equally wants ſuch additional warmth.

With regard to the torpidity of this bird, M. de Buffon relies upon Linnæus's aſſerting, that *ſæpe ſepelitur in nive* [*b*]; which by no means ſignifies that the bird is torpid, but only that it buries itſelf, ſometimes, under the ſnow; as ſheep do with us in the more rigorous ſeaſons, when it lies very deep in the mountains.

[*a*] T. ii. p. 216.

[*b*] Linn. S. Nat. p. 159. This circumſtance is alſo obſerved by Pontoppidan, Pt. ii, p. 75. Engl. Tranſl.

The Black Cock, however, is ſo far from being torpid in the winter, that it even approaches the habitation of man when diſtreſſed for food; and I ſhall likewiſe conclude, till I ſee a ſpecimen which proves the contrary, that, like the other Tetraos, whoſe feet are covered low with feathers, this part of the plumage becomes thicker in winter.

M. de Buffon alſo ſeems to be miſtaken in ſuppoſing, that the thick plumage round the feet is peculiar to the Lagopus; as it is believed, that Linnæus's firſt diviſion of this genus have all of them the ſame additional cloathing for the winter; nor is this extraordinary warmth confined merely to this genus, as the noble ſpecimen of the large White Owl, which hath lately been preſented to the Royal Society from Hudſon's Bay, is covered about the claws with a plumage of perhaps an equal thickneſs.

The next remarkable circumſtance in this bird is, that the ſhafts of many of the wing-feathers are black; which M. de Buffon ſuppoſes to be only ſix; whereas they are eight in the ſpecimens from Hudſon's Bay; the two laſt are, indeed, of a fainter colour.

M. de Buffon next ſays [*c*], that Briſſon counts eighteen feathers in the tail; and Willoughby, ſixteen; which he reduces himſelf to fourteen. It ſeems to me, however, that Willoughby's number is the more accurate; and, by examining the difference between the ſummer and winter ſpecimens, I find that the black feathers of the tail are covered by two upper ones, which in ſummer are brown, and in winter white.

[*c*] T. ii. p. 264.

I cannot

I cannot alſo diſcover, in any of the ſpecimens, the two white feathers in the tail, according to Linnæus's deſcription, *rectricibus nigris apice albis, intermediis albis*, as the two covering feathers beforementioned cannot, with propriety, be termed *intermedii*; nor are they white in the ſummer, but brown: ſo that Linnæus makes a circumſtance, which varies with the ſeaſon, to be a permanent characteriſtic of the bird.

M. de Buffon next ſuppoſes [*d*], that Willoughby and Friſch ſpeak of different birds under the name of Lagopus; becauſe the firſt ſays, that the feet are covered with ſoft, and the latter, with harſh and briſtly feathers. The remarks, however, of theſe ornithologiſts, are eaſily reconciled; for, if the finger is drawn according to the courſe of the feathers, they feel ſoft; and, if in the contrary direction, harſh and briſtly. The difference alſo between Belon, Geſner, and Linnæus, with regard to the call of this bird, is as eaſily accounted for; becauſe moſt male birds differ from the female in this reſpect, and ſometimes the young birds from thoſe which are full-grown.

This naturally brings me to ſhew, that M. de Buffon (who hath great merit in other parts of his Natural Hiſtory, by not unneceſſarily multiplying the ſpecies of animals,) hath, in this kind of Tetrao, conſidered as two ſpecies what, when properly examined, will turn out to be only the Lagopus, or Ptarmigan.

His chief reaſon for conſidering the Lagopus of Hudſon's Bay, as being diſtinct from the Ptarmi-

[*d*] T. ii. p. 271.

gan, ariſes from his aſſerting, that Mr. Edwards, in his deſcription of that bird, ſays, that it is twice as large. Mr. Edwards, however, only conſiders the ſize of the Hudſon's Bay Lagopus as between that of a Pheaſant and a Partridge; in which he is very accurate: the bird is not only evidently ſo to the eye, but weighs three ounces more than a common Partridge [*e*].

M. de Buffon likewiſe ſeems to make an unneceſſary ſpecies of Tetrao, under the name of *le petit Tetras, à plumage variable*; as his principal argument for this opinion is, that they are not found on the mountains, as the Lagopi are.

Now, it is very clear, from the name given in the catalogue from Hudſon's Bay to this bird, of the *Willow Partridge*, that it lives entirely in that part of the world on the plains; nor are there (it is believed) any very high mountains in the neighbourhood of our forts.

When M. de Buffon, therefore, conceives, that the Lagopus is always endeavouring to find out ſnow and ice, and that it carefully avoids the glare of the ſun [*f*]; it ſhould ſeem, that the obſervation is by no means generally true; becauſe, though the rigour of a Hudſon's Bay winter is great, yet the ſummer is very pleaſant, and the ſnow ſoon diſappears, without which M. de Buffon imagines that the bird cannot exiſt; though his ninth plate repreſents the Ptarmigan, in his winter dreſs, ſur-

[*e*] The Partridge, when full-grown, weighs thirteen ounces, and the Ptarmigan, ſixteen.

[*f*] T. ii. p. 272.

rounded

rounded with trees and plants in moſt luxuriant foliage and vegetation.

I cannot agree, moreover, with M. de Buffon, when he ſays, that the fleſh of the Lagopus is bitter; as I have eat them myſelf in the Highlands of Scotland; nor ſhould I have been able to diſtinguiſh the taſte from that of the common Grous, which is well known to be a bird of moſt excellent flavour.

What I have hitherto remarked is chiefly in anſwer to that ingenious naturaliſt, M. de Buffon, who is the laſt ornithologiſt that hath both deſcribed and engraved this bird. I have only one new obſervation to make myſelf; which is, that the claws are ſcooped off at the end exactly like a writing-pen (wanting indeed the ſlit); which circumſtance may likewiſe be ſeen in the claws of our common Grous, or Heath-game, though the reſemblance is not quite ſo ſtrong as in the Ptarmigan.

I ſhall now conclude with copying, from the catalogue tranſmitted with the ſpecimens from Hudſon's Bay, what further relates to the Lagopus; which, as I obſerved before, is there called a Willow-partridge [g].

" The Willow-partridges gather together in large " flocks in the beginning of October, harbouring " amongſt the willows, the tops of which are their " principal food; they then change to their winter

[g] It is not at all extraordinary, that it ſhould there be conſidered as a Partridge; becauſe the White Partridge is the name given to this bird by the old ornithologiſts, who have very naturally conſidered edible birds nearly of the ſame ſize, as Partridges when they have ſhort tails, and as Pheaſants, when they have long ones.

" dreſs. They change again in March, and have
" their complete ſummer dreſs by the latter end of
" June. They make their neſt in the ground in
" dry ridges; and are ſo plentiful, that ten thou-
" ſand have been killed in the three forts in one
" winter."

I am, dear SIR,

Your moſt faithful,

humble Servant,

Daines Barrington.

XXIX. *Ac-*

XXIX. *Account of the Effects of Lightning at* Steeple Aſhton *and* Holt, *in the County of* Wilts, *on the 20th of* June, 1772, *contained in ſeveral Letters, communicated by* Edward King, *Eſq*; *F. R. S.*

To Edward King, Eſq;

SIR,

Read March 18, 1773. I Have ſent you, agreeably to your requeſt, ſome account of a ſtorm of thunder and lightning that happened at Steeple Aſhton, in Wiltſhire, on the 20th of June laſt. I was from home when it happened; but the truth of it is atteſted by the Reverend Mr. Wainhouſe of Steeple Aſhton, and the Reverend Mr. Pitcairn of Trowbridge, who were in my houſe during the tempeſt, and were in danger of loſing their lives by it. I have added a deſcription of the effects of the ſame ſtorm upon a houſe at Holt, in this county. Both tend to prove in a remarkable manner the danger of placing any conſiderable quantity of iron in the upper part of chimnies, without a conductor to guard againſt the ſtrokes of lightning. You are at liberty to lay theſe papers before

before the Royal Society, if you think them deſerving of their attention.

I am,

SIR,

Your obedient

humble Servant,

Steeple Aſhton,
Aug. 10, 1772.

L. ELIOT, Vicar of Steeple Aſhton, in Wiltſhire.

ON the 20th of June, 1772, between twelve and one o'clock in the afternoon, a violent ſtorm of thunder and lightning happened at Steeple Aſhton, in Wiltſhire. During the ſtorm, a woman in the village ſaw a large quantity of lightning come out of a cloud, part of which is ſuppoſed to have fallen on the top of the north chimney of the vicarage houſe, attracted probably by an iron hoop that went round the chimney, and by ſome iron bars placed within it, that formerly made part of an apparatus to prevent its ſmoking. That the lightning fell on theſe iron bars is very probable, becauſe the colour of two of them that were contiguous was changed, nine or ten inches in length, to a dark blue, like that of a watch ſpring, no uncommon effect of electrical fire.

In the north parlour, to which this chimney belonged, were the Reverend Mr. Wainhouſe, of Steeple Aſhton, and the Reverend Mr. Pitcairn, of Trowbridge, the former ſtanding, and the latter

ter ſitting in a great chair, with his back to the fire-place, near the wire of a bell. In the ſouth parlour, ſeparated from the other by a hall, were a maid ſervant and a painter; in the kitchen another maid ſervant; in the coal-houſe, four or five yards from the houſe, a man ſervant; near the barn, about fifty or ſixty yards from the houſe, another man ſervant. When the lightning fell upon the houſe, the man ſervant near the barn heard a very loud noiſe, equal, he ſuppoſes, to the ſound of twenty cannons fired at once, and would have fallen to the ground, if he had not caught hold of ſomething to ſupport himſelf. The other man ſervant in the coal houſe was ſtruck backward, and felt ſomething, as he deſcribes it, like a ſtream of warm water poured upon the middle of his body, which, if it was not the electric fluid itſelf, was the heated air expanding itſelf with violence after the exploſion. The maid in the kitchen heard a great noiſe, but received no ſhock. The other maid ſervant, who was ſtanding near *the middle* of the ſouth parlour, ſuffered likewiſe no ſhock, being only terrified exceedingly with the exploſion, and the ſparks of fire, which ſhe ſaw on all ſides of her; but the painter, who was in the ſame room, painting near the chimney and the bell wire, was ſtruck on the left ſide of his body that was next the wire, from his head to his waiſt; he felt in particular a ſevere ſhock, like the electrical one, in his left wriſt, which was marked all round with blue and yellow intermixed; a ſplinter from the wooden caſe, that covered the bell-wire, ſtruck through his glove, and wounded his hand; and he was ſtunned for ſome time.

It may be proper to obſerve, that immediately after the woman had ſeen the lightning come from the cloud, as above-mentioned, ſome perſons in the village, beſides thoſe in or near the vicarage houſe, were thrown to the ground.

The following is the account, which Mr. Wainhouſe and Mr. Pitcairn give of what happened in the north parlour in which they were. As they were converſing about a loud clap of thunder that had juſt happened, they ſaw on a ſudden a ball of fire between them, upon a level with the face of the former, and about a foot from it. They deſcribe it to have been of the ſize of a ſixpenny loaf, and ſurrounded with a dark ſmoke; that it burſt with an exceeding loud noiſe, like the firing of many cannons at once; that the room was inſtantly filled with the thickeſt ſmoke; and that they perceived a moſt diſagreeable ſmell, reſembling that of ſulphur, vitriol, and other minerals in fuſion; inſomuch that Mr. Pitcairn thought himſelf in danger of ſuffocation. Mr. Wainhouſe providentially received no hurt, except a ſlight ſcratch in his face from the broken glaſs that was flying about the room, a kind of ſtupefaction for ſome time, and a continued noiſe in his ears, which noiſe, the effect of the exploſion, happened likewiſe to Mr. Pitcairn, and others in the houſe.

The lightning fell on Mr. Pitcairn's right ſhoulder, made a hole in his coat, about a quarter of an inch in diameter, went under his arm in one line to his breaſt, deſcended from thence down the lower parts of his body in two irregular lines, about half an inch broad, attracted probably by

his watch, the glaſs of which it ſhivered into ſmall pieces, and meeting perhaps with a little reſiſtance from it, ſpread itſelf round his body, and produced the ſenſation of a cord, tied cloſe about his waiſt. A violent pain in his loins immediately followed; and from thence to his extremities there ſeemed to be a total ſtoppage of circulation, all ſenſation being loſt, and his legs and feet reſembling in colour and appearance thoſe of a perſon actually dead. Beſides ſhivering the glaſs of his watch, the lightning melted a little of the ſilver of it, and a ſmall part alſo of half a crown in his pocket. When it came to the middle of his thigh, it left an impreſſion of a blackiſh colour, reſembling the branch of a tree, which in a few days diſappeared; but the lines on his body are ſtill viſible, and are of a dark blue, intermixed irregularly with a deep yellow. From the middle of his thigh the lightning changed its direction again, and went down the under ſide of it to the calf of his leg, and ſo to his ſhoe, which *was ſplit into ſeveral pieces* in ſo remarkable a manner, as juſtly to claim the inſpection of the curious. As ſoon as Mr. Pitcairn was ſtruck, he ſunk in his chair, but was not ſtunned; his face was blackened, and the features of it diſtorted. His body was burned in ſeveral places, ſmall holes were made in different parts of his cloaths, and he loſt in ſome meaſure the uſe of his legs for two or three days; but by proper care he ſoon recovered, except a weakneſs and numbneſs in his right leg, which ſtill remains. What is remarkable, Mr. Pitcairn remembers very well to have ſeen the ball of fire in the room for

a ſhort time, a ſecond or two, *after he found himſelf ſtruck with the lightning*. Extraordinary as this circumſtance may appear, it may be proper to take notice, that it is entirely agreeable to an obſervation of the learned and ingenious Dr. Franklin, quoted below *.

The effects of the lightning on the building and furniture were as follows. The north chimney was thrown down, the roof and cieling near it beat in; large ſtones were forced out of the walls, ſome were driven to a conſiderable diſtance, one in particular to about 200 feet. The glaſs of the windows in the north parlour and the chamber over it was forced outwards, except in the caſements, which were open, and in which not a pane of glaſs was broken. The caſe of a clock in the ſame parlour fell forwards, and was beaten to pieces; a looking glaſs over the chimney was thrown on the floor, and broken, ſome of the quickſilver was melted, as was likewiſe ſome of the lead belonging to the windows. A bureau, that was locked, was opened; as was alſo the parlour door, inwards, probably by the external air ruſhing in to reſtore the equilibrium. Some bedding in one of the chambers was fired, but the fire was extinguiſhed of itſelf, or by the rain that fell during the ſtorm, before it was diſcovered. Several ſplinters were torn out of a hogſhead full of

* In every ſtroke of lightning I am of opinion that the ſtream of the electric fluid, &c. will go conſiderably out of a direct courſe for the ſake of the aſſiſtance of good conductors; and that in this courſe it is actually moving, though ſilently and imperceptibly, *before the exploſion*, in and among the conductors, &c. Franklin's Experiments and Obſervations on Electricity, edit. 4. pag. 124.

beer

beer, but the caſk was not materially damaged, nor the beer ſpilt. The iron bell-wire in both the parlours and the hall was reduced to ſmoke and entirely diſſipated, excepting in thoſe parts where it was twiſted, and double, and alſo the wire ſprings contiguous to the bell, which the lightning left undamaged, as well as the braſs handles and bell itſelf. The cieling and wall on each ſide, where the wire went, was ſtained irregularly, a foot or more in breadth, with a dark blue intermixed with a deep yellow. It is worth obſerving, that this iron bell wire was very ſmall, conſiderably leſs than a common knitting needle; but though it was itſelf deſtroyed, yet it ſeems to have ſerved as a conductor to the lightning, and to have prevented worſe effects than happened. For when the lightning had run along, and conſumed all the *ſingle* wire, and had reached that which was twiſted and double in the ſouth parlour, contiguous to the braſs handle, which the bell uſed to be rung with, it made a hole in the wall of five or ſix inches in diameter, being attracted probably by an iron ſtove on the other ſide in the kitchen chimney, where meeting with ſeveral large conductors, andirons, poker, tongs, &c. it ſeems to have been conveyed into the ground. This appears probable, becauſe the progreſs of it below ſtairs could not be traced beyond this hole, which it made in the wall. In the chamber over the kitchen, a ſmall piece of wood was indeed ſtruck out of a bed poſt, and the glaſs of half a window was driven outwards; but this does not ſeem to have been the immediate effect of the lightning, but of the ſhake from the exploſion.

Upon the whole, it muſt be allowed, that Mr. Wainhouſe and Mr. Pitcairn had a moſt wonderful and providential eſcape, particularly the laſt gentleman; for had the lightning paſſed from his ſhoulder in a right line through his body, inſtead of going round upon the ſurface of it, under his arm, in order to come at his watch, by which it ſeems to have been attracted, immediate death would in all probability have been the conſequence.

Whether Steeple Aſhton is from its ſituation particularly expoſed to thunder-ſtorms, is uncertain. It may however be proper to mention, that in the year 1670, July the 25th, a violent ſtorm of thunder and lightning damaged the church ſteeple, which was 93 feet high; and on the 15th of October in the ſame year, another thunder ſtorm threw it entirely down, and killed two of the workmen, who were repairing it.

We have peruſed, and carefully examined the above account, and hereby teſtify the truth of all the facts related in it, to the beſt of our knowledge and belief.

Auguſt 22,
1772.

William Wainhouſe.
Robert Pitcairn.

To Edward King, Eſquire.

P. S.

P. S.

I fear the deſcription I left at your houſe of the thunder ſtorm laſt June is too long; you may ſhorten it as much as you think proper. I purpoſely omitted a few circumſtances, which I thought leſs material. Since I was in London, I have been informed of two particulars, which I will beg leave to mention to you.

Mr. Field, a painter of Trowbridge, during the ſtorm, obſerved a ball of fire vibrate forward and backward in the air over ſome part of Steeple Aſhton, and at laſt dart down perpendicularly, which in all probability was the ball of fire that Mr. Wainhouſe and Mr. Pitcairn ſaw in the north parlour of the vicarage houſe.

The other circumſtance is as follows: After the exploſion of the ball of fire in the north parlour, Mr. Pitcairn obſerved a great quantity of fire of *different* colours *vibrating* in the room forwards and backwards with a moſt extraordinary ſwift motion.

The vibration in both theſe caſes obſerved by different perſons before and after the exploſion is a fact, which I ſhould have taken notice of, had I been made acquainted with it ſooner.

I am, dear Sir,

Your obliged and affectionate

humble ſervant,

Steeple Aſhton,
near Trowbridge, Wilts,
Nov. 28, 1771.

L. Eliot.

To

To the Rev. Mr. Eliot.

SIR,

Hearing of the damage done by lightning to your houſe, on the 20th of June laſt, I have ſent you an account of what happened to mine on the ſame day, and nearly at the ſame time.

During the ſtorm a perſon in this place ſaw a body of fire moving towards a houſe that is next to mine, though at ſome diſtance from it; attracted probably by a large iron bar of ten or twelve feet long, fixed horizontally to ſupport a high chimney. This body of fire changed its direction, and fell on my houſe, forced a brick out of the chimney, near that part of it to which the iron bar was faſtened, and went through the houſe to an outward door on the oppoſite ſide, which happened to be open; there it burſt with a loud noiſe, like the firing of cannons, and filled the room where I was with ſmoke and the ſmell of ſulphur. I was fortunately three or four feet out of the line in which it moved. I was however ſtruck againſt the wall near which I ſtood; my body was covered with fire, and I thought for ſome time I ſhould have been ſuffocated with ſmoke and the ſmell of ſulphur; but by the bleſſing of Providence I eſcaped unhurt, and my houſe received no damage.

I reſpectfully am,

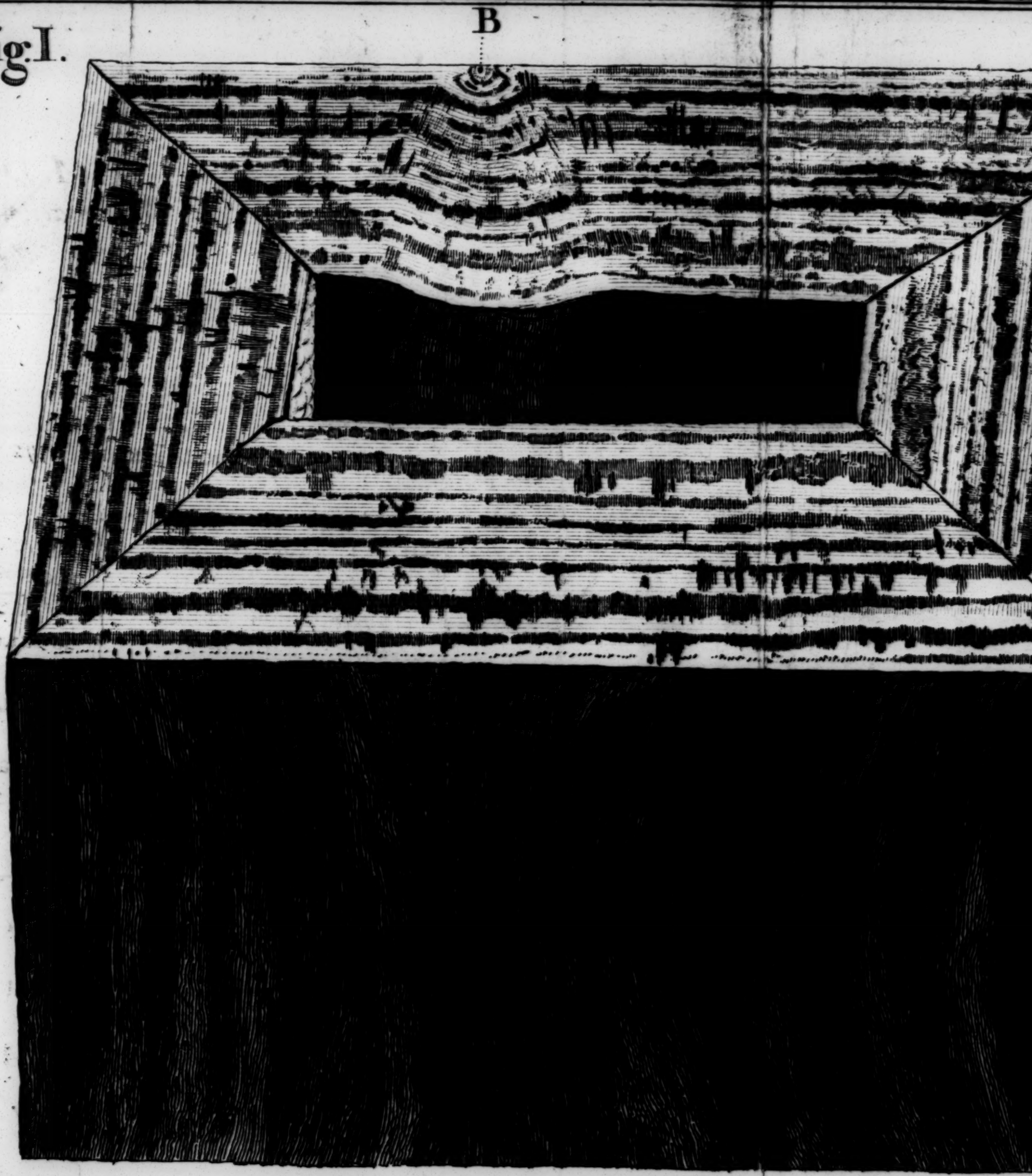
SIR,

Your moſt humble Servant,

William Paradiſe.

Holt, Auguſt 22, 1772.

Fig. I.





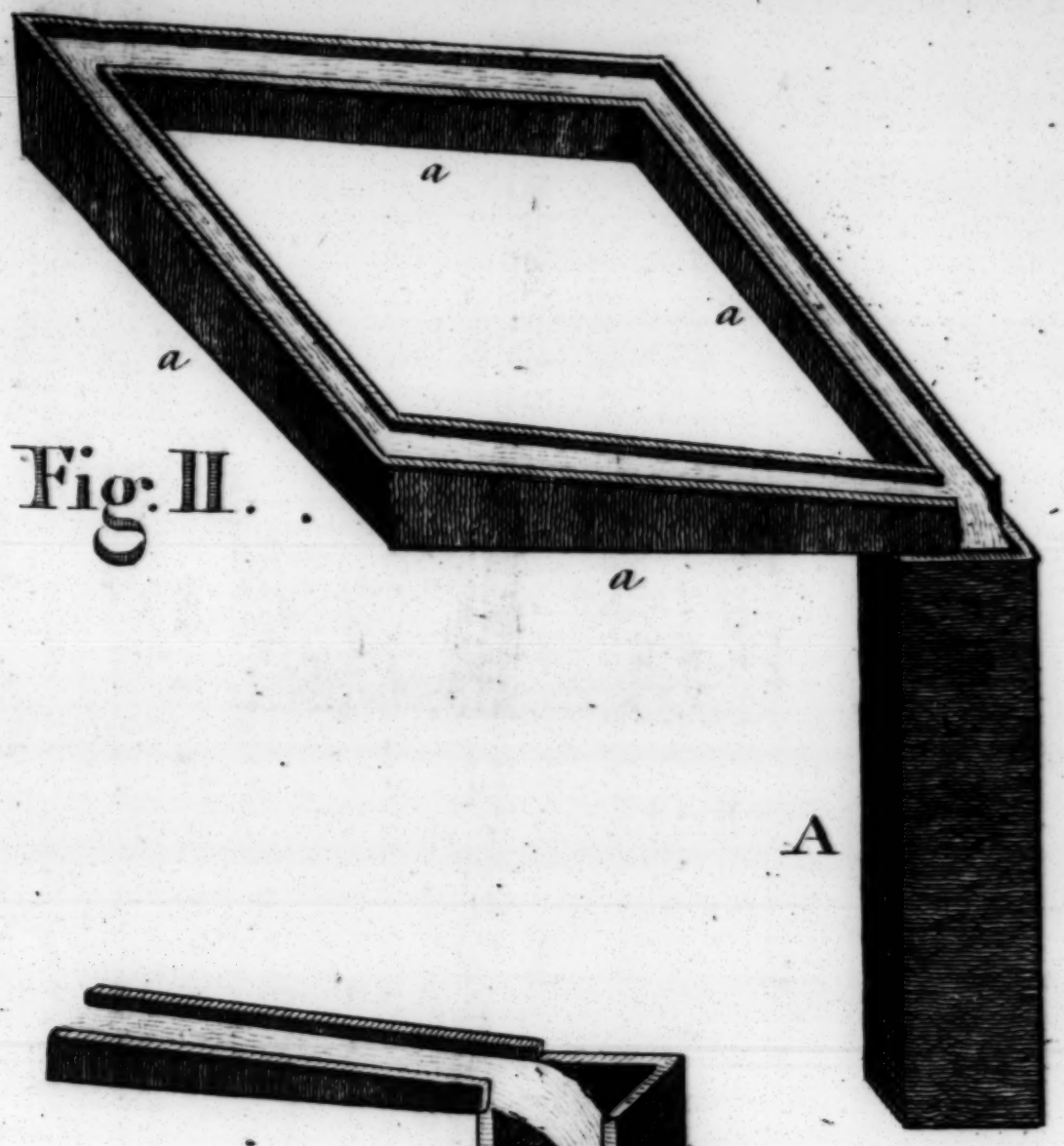
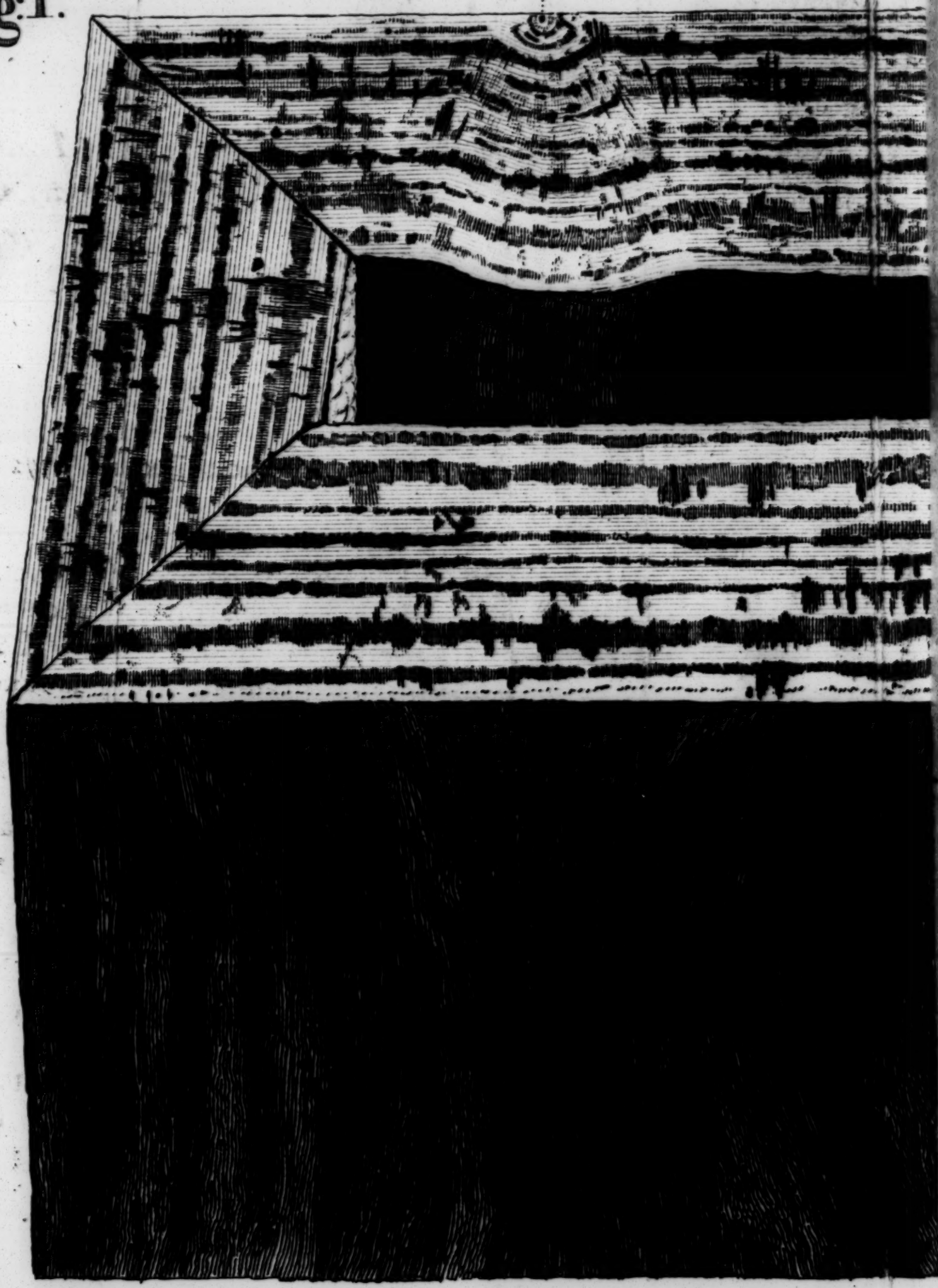


Fig: II.



Fig: III.

Fig. I.



XXX. *A Letter to* Mathew Maty, *M. D. Sec. R. S.*; *containing ſome Obſervations on a ſingular Sparry Incruſtation found in* Somerſetſhire. *By* Edward King, *Eſquire, F. R. S.*

John Street, Dec. 18, 1772.

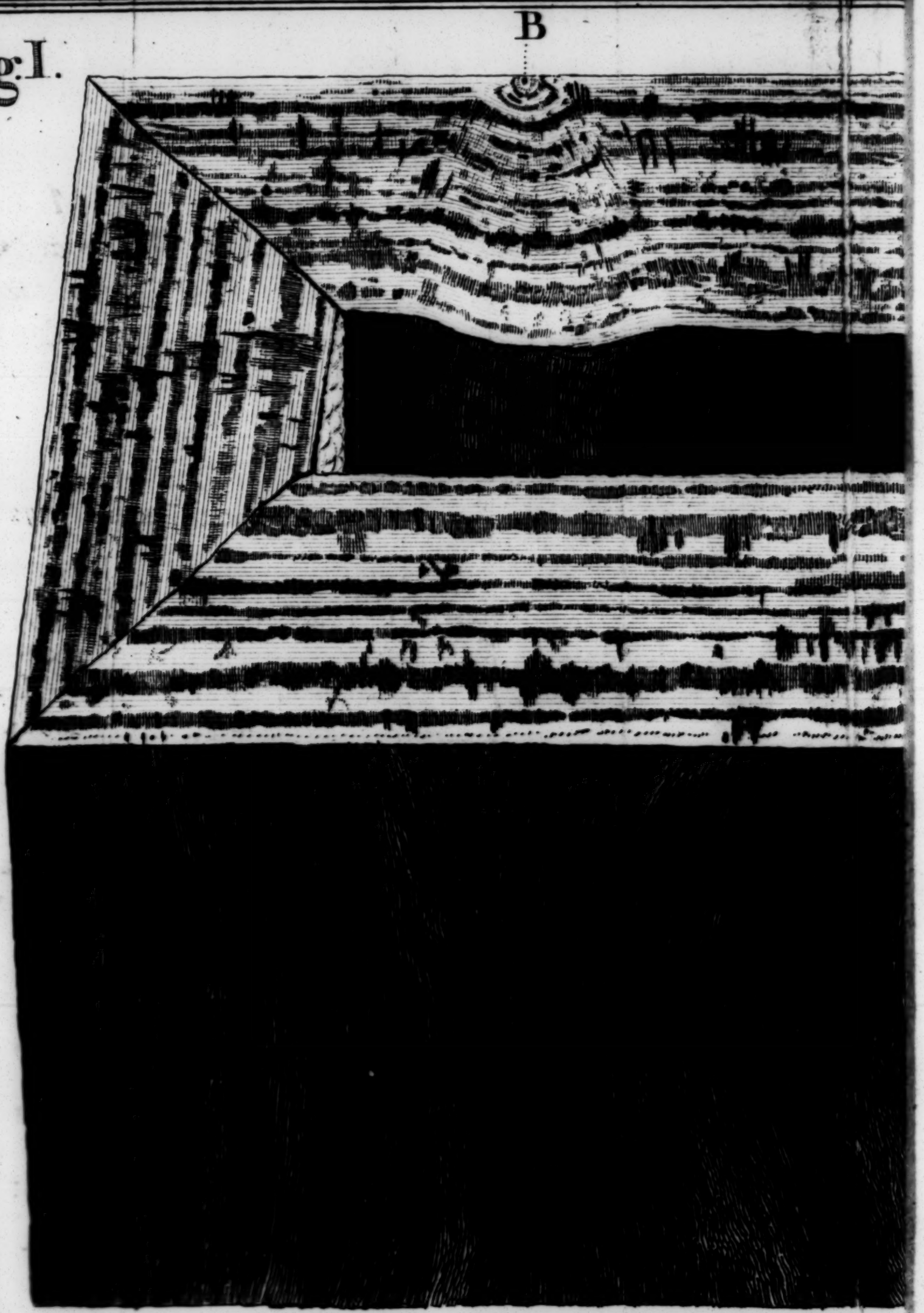
SIR,

Read March 25, 1773.

THE ſummer before laſt, when I was in Somerſetſhire, I received a preſent of a very curious foſſil from the Rev. Mr. Catcott of Briſtol, a gentleman who has taken much pains to collect the foſſil bodies of that County, and is very indefatigable in his reſearches. And as ſome few ſingular obſervations occur to me with regard to this extraordinary maſs, I take the liberty to trouble you with them; and requeſt, if you think them at all deſerving any conſideration, that they may be laid before the Society, together with a drawing of the foſſil, (Tab. X.) and with the very exact account I received of its production from Mr. Catcott.

In the pariſh of High Littleton, in the County of Somerſet, ſituated about midway between Briſtol and Wells, are ſeveral Coal-mines; and about the end of the year 1766, a new ſhaft (or pit) was opened,

Fig. I.



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In the pariſh of High Littleton, in the County of Somerſet, ſituated about midway between Briſtol and Wells, are ſeveral Coal-mines; and about the end of the year 1766, a new ſhaft (or pit) was opened,

for the purpoſe of conveying air into an adjoining work, called Mearn's coal-pit; but when this ſhaft was finiſhed, the water that flowed in from the ſides, and which at firſt was taken up by buckets, greatly incommoded the under-works; and therefore, to prevent this inconvenience, the miners, at about the depth of ten fathom, and juſt below the place where the water broke in, affixed to the four ſides of the pit ſome wooden ſhoots (as repreſented at Fig. 2.), about four or five inches wide, and as many deep; all of them a little inclined towards one corner, where was a hollow perpendicular pipe or trunk of elm, nearly a long ſquare (as repreſented at A), being about ſeven inches and an half one way, and four inches and an half the other; and through this the water, that fell into the lateral ſhoots, was conveyed down to the level (or paſſage out); which being about ſeven fathoms lower than the ſhoots, the hollow perpendicular trunk was about fourteen yards in length.

This trunk having been thus fixed up, in the latter end of the year 1766, was in about three years time, or rather leſs, found to be much obſtructed, and ſtopped up; ſo that, in Auguſt 1769, the miners were obliged to take it up: and then, on examining it, and taking it to pieces, they found the whole cavity, from one end to the other, nearly filled with a ſort of ſparry incruſtation, ſomewhat ſofter than marble, but harder than alabaſter, and which therefore I ſhall venture to call a ſpecies of marble. And the ſpecimen now laid before the Society, and repreſented by the drawing (Fig. 1.), and alſo another ſpecimen of the like ſort, preſented to the Society,

are merely tranſverſe ſections of the ſubſtance, with which the pipe was filled.

The water, that flowed into the pit on all ſides, iſſued from a ſtratum of hard brown and reddiſh ſand-ſtone, replete with ſhining ſparry *micæ*, and ſome ocherous matter; and had, in its paſſage through the trunk, regularly filled up the cavity, by ſlow degrees, with ſolid incruſtations; inſomuch, that the increaſe of the marble is marked much in the ſame manner as the increaſe of the growth of a tree appears to be, when the trunk of it is cut horizontally: and at laſt the water had left only the cavity, which now appears in the middle of the block, and which was uniform in its figure from one end of the pipe to the other, and nearly ſimilar to the original cavity; but which, at laſt, not being large enough to let all the water paſs, occaſioned the diſcovery. Since that time, in order to prevent the inconvenience (if poſſible) a new trunk has been made, larger than the firſt; and yet, in June 1771, this new trunk alſo was ſo far filled up with the ſparry incruſtation, that there was but juſt room to thruſt four fingers into the central cavity; and the lateral ſhoots, or troughs, alſo have filled ſo faſt, that they have been obliged, every now and then, to clean them out.

This, Sir, is the hiſtory of the ſpecimen now laid before the Society, and of the Mine from whence it came; and the obſervations I would beg leave to make, are the following.

1ſt, As the water flowed in from the ſhoots, on two ſides of the ſquare trunk or pipe, it is manifeſt that the ſtreams muſt have ſtricken againſt each

other, at the corner of the pipe where they firſt met, and alſo at the oppoſite corner. And, as it is a known principle of mechanics, that a body, which is acted upon by two forces moving in different directions, will deſcribe the diagonal of a parallelogram, of which the directions of thoſe forces ſhall be the ſides; ſo here, the line in which the two ſtreams met, and impeded each other's motion, has plainly, as the marble increaſed, gone on in the diagonal of ſuch a parallelogram from both the corners; viz. from that where the pipe joined the ſhoots, or troughs, and from the oppoſite one: but it is alſo very remarkable, that there is ſuch a diagonal line, not only at theſe corners, but in like manner at the other two; which can be accounted for no otherwiſe, than by ſuppoſing that each of the two ſtreams, daſhing againſt the oppoſite ſide of the pipe, formed continually, the whole way down, another ſtream, in a contrary direction, as repreſented in Fig. 3; and ſo, both together, produced the ſame effect throughout the whole pipe, as if there had been four ſtreams flowing over the four ſides. Upon examining the block, however, very ſtrictly, it appears, that the lines in the diagonal one way, are ſtronger than thoſe in the diagonal the other way; and indeed the ſpccimen of the pipe, preſented to the Society, has even broken in halves, exactly in one of the diagonals, though the block here deſcribed remains intire, and has the appearance of having had its ſides joined accurately, in the manner in which a ſkilful workman would fit four boards to be glued together.

2dly,

2dly, At the place marked B, Fig. 1, there ſeems to have been, by ſome accident or other, the point of a ſmall nail projecting into the pipe; and here, it is very remarkable, that, either by the daſhing of the water, or rather perhaps by an effect which iron has been obſerved to have of haſtening and increaſing petrification, the incruſtation has gone on faſter than in other parts of the ſame ſide; but ſo regularly, that, from the point of the nail to the inner cavity, there is a ſwelling, or protuberance, ſo uniform, that it makes throughout nearly the ſame ſegment of different circles, of which the point of the nail is the common center; and that not merely directly oppoſite to the nail, but throughout this whole block and even further downwards.

3dly, The regular increaſe of theſe ſegments of circles is viſible in each *lamina* of the block (if I may be allowed that expreſſion), and in each *lamina* the diameter of the circle increaſes in due proportion; ſo that it is ſtill nearly the ſame ſegment; though, if there be any difference, it is rather a ſmaller portion of a larger circle; as, from the cauſe which occaſioned it, one would be led to expect. And with regard to theſe *laminæ*, it is worth obſerving, that as they mark the increaſe of the marble uniformly all round, as the growth of a tree is marked (only the marble increaſed inward, whereas a tree grows outward) ſo they ſeem to have become viſible, and to have been thus diſtinctly marked, by means of the water bringing, at different times, more or leſs oker along with the ſparry matter: and this is the more probable, as the whole country all round abounds

with beds of oker, and the waters are ſometimes much tinged with it.

4thly, The cavity left in the middle of the block is not perfectly ſimilar to the original cavity of the trunk, or pipe; becauſe the water did not flow quite uniformly over the edges, at the ends of the ſhoots or troughs, in conſequence, probably, of their not lying exactly horizontally: whence, more water fell upon and againſt one part of the ſides of the trunk, than againſt the other.

5thly, The outſide of the block has taken off impreſſions of all the roughneſſes, knots, and ſhivers of the elm boards, which compoſed the trunk or pipe, even more accurately than they could have been taken off by wax, plaiſter of Paris, or almoſt any compoſition whatever, and certainly much more durably: which impreſſions, although they are not ſo well repreſented in the drawing, in conſequence of their fineneſs, yet appear ſufficiently plain, both on the ſpecimen here deſcribed, and on that preſented to the Society, and are exceedingly well deſerving of notice.

There is in the Philoſophical Tranſactions (Vol.LX. p. 47.) a very curious paper, from our learned foreign Member R. E. Raſpe, concerning the production of white marble in a ſimilar manner; in which paper he mentions the taking off impreſſions of medallions, by means of petrifying waters. And I remember a paper was read at the Royal Society ſome time ago, containing an account of ſeveral impreſſions, actually ſo taken off in a ſhort time, in durable marble, by means of a petrifying water, near Bologna in Italy: when ſome of the impreſſions were alſo ſent, both to the

the Royal Society, and to the Britiſh Muſeum. And, as this block here deſcribed, and the whole contents of the pipe, of above forty feet in length, were formed in leſs than three years, there is reaſon to conclude, that the water of this Mine in Somerſetſhire is as capable of being improved to the purpoſes of a new manufactory, as either that near Bologna, or thoſe of Germany and Bohemia. And it is perhaps worth mentioning, that ſomething of this ſort has actually been attempted, with good ſucceſs, in Peru; for we are told by P. Feuillée (who made ſeveral curious obſervations in South America, both phyſiological and aſtronomical, in 1709), that he ſaw many ſtatues and beautiful vaſes (or holy water pots), in the churches at Lima, which were ſimply caſt in moulds, by means alone of a petrifying water near Guankabalika, or Guankavelika. And this circumſtance is alſo mentioned (p. 236.), in a Deſcription of Peru, publiſhed in 1748, a great part of which is taken from Feuillée's account.

6thly, This block of marble takes a very fine poliſh, as appears by the ſpecimen, the ſections of which are poliſhed; and if caſts of medals, or other things, were taken in ſmooth moulds, well formed, their ſurfaces would, therefore, probably appear well poliſhed, as thoſe of the medals did, which came from Bologna.

7thly and laſtly, I would only add, that Dr. Pococke, in his Travels (Vol. II. p. 264.), deſcribing a very curious grotto in the iſland of Candia, or Crete, which exceeded all others that he ever ſaw in beauty, and the ſlenderneſs of the pillars, one of which is near twenty feet high, and even tranſparent,

ſays,

ſays, " As I had ſeen ſtones of this kind hewn out " of a rock at Mount Lebanon, which were uſed as " white marble, and appeared to be alabaſter, this " made me imagine, that when theſe ſorts of petri- " factions are hard enough to receive a poliſh, they " then become the oriental tranſparent alabaſter, " which is ſo much valued, and of which there are " two curious columns at the high altar of St. Mark " in Venice." Perhaps Dr. Pococke does not here ſufficiently diſtinguiſh between marble and alabaſter; but I add his remark, merely to ſhew how valuable theſe incruſtations may become, and how much they deſerve not to be neglected.

I am,

SIR,

Very reſpectfully,

Your moſt obedient,

humble ſervant,

Edward King.

XXXI. *Experiments and Obſervations on the Singing of Birds, by the Hon.* Daines Barrington, *Vice Preſ. R. S. In a Letter to* Mathew Maty, *M. D. Sec. R. S.*

Read April 22, May 6, and May 13, 1773.

January 10, 1773.

DEAR SIR,

AS the experiments and obſervations I mean to lay before the Royal Society relate to the ſinging of birds, which is a ſubject that hath never before been ſcientifically treated of *, it may not be improper to prefix an explanation of ſome uncommon terms, which I ſhall be obliged to uſe, as well as others which I have been under a neceſſity of coining.

To *chirp*, is the firſt ſound which a young bird utters, as a cry for food, and is different in all neſtlings, if accurately attended to; ſo that the hearer may diſtinguiſh of what ſpecies the birds are, though the neſt may hang out of his ſight and reach.

* Kircher, indeed, in his *Muſurgia*, hath given us ſome few paſſages in the ſong of the nightingale, as well as the call of a quail and cuckow, which he hath engraved in muſical characters. Theſe inſtances, however, only prove that ſome birds have in their ſong, notes which correſpond with the intervals of our common ſcale of the muſical octave.

This

This cry is, as might be expected, very weak and querulous; it is dropped entirely as the bird grows ſtronger, nor is afterwards intermixed with its ſong, the *chirp* of a nightingale (for example) being hoarſe and diſagreeable.

To this definition of the *chirp*, I muſt add, that it conſiſts of a ſingle ſound, repeated at very ſhort intervals, and that it is common to neſtlings of both ſexes.

The *call* of a bird, is that ſound which it is able to make, when about a month old; it is, in moſt inſtances (which I happen to recollect), a repetition of one and the ſame note, is retained by the bird as long as it lives, and is common, generally, to both the cock and hen *.

The next ſtage in the notes of a bird is termed, by the bird-catchers, *recording*, which word is probably derived from a muſical inſtrument, formerly uſed in England, called a recorder †.

This attempt in the neſtling to ſing, may be compared to the imperfect endeavour in a child to babble. I have known inſtances of birds beginning to *record* when they were not a month old.

* For want of terms to diſtinguiſh the notes of birds, Bellon applies the verb *chantent*, or ſing, to the gooſe and crane, as well as the nightingale. "Pluſieurs oiſeaux *chantent* la nuit, comme "eſt l'oye, la grue, & le roſſignol." Bellon's Hiſt. of Birds, p. 50.

† It ſeems to have been a ſpecies of flute, and was probably uſed to teach young birds to pipe tunes.

Lord Bacon deſcribes this inſtrument to have been ſtrait, to have had a leſſer and greater bore, both above and below, to have required very little breath from the blower, and to have had what he calls a *fipple*, or ſtopper. See his ſecond Century of Experiments.

This

This firſt eſſay does not ſeem to have the leaſt rudiments of the future ſong; but as the bird grows older and ſtronger, one may begin to perceive what the neſtling is aiming at.

Whilſt the ſcholar is thus endeavouring to form his ſong, when he is once ſure of a paſſage, he commonly raiſes his tone, which he drops again when he is not equal to what he is attempting; juſt as a ſinger raiſes his voice, when he not only recollects certain parts of a tune with preciſion, but knows that he can execute them.

What the neſtling is not thus thoroughly maſter of, he hurries over, lowering his tone, as if he did not wiſh to be heard, and could not yet ſatisfy himſelf.

I have never happened to meet with a paſſage in any writer, which ſeems to relate to this ſtage of ſinging in a bird, except, perhaps, in the following lines of Statius:

" ———Nunc volucrum novi
" Queſtus, inexpertumque carmen,
" Quod tacitâ ſtatuere brumâ."
Stat. Sylv. L. iv. Ecl. 5.

A young bird commonly continues to *record* for ten or eleven months, when he is able to execute every part of his ſong, which afterwards continues fixed, and is ſcarcely ever altered.

When the bird is thus become perfect in his leſſon, he is ſaid to *ſing his ſong round*, or in all its varieties of paſſages, which he connects together, and executes without a pauſe.

I would therefore define a bird's *ſong* to be a ſucceſſion of three or more different notes, which are continued without interruption during the ſame interval with a muſical bar of four crotchets in an adagio movement, or whilſt a pendulum ſwings four ſeconds.

By the firſt requiſite in this definition, I mean to exclude the call of a cuckow, or *clucking* of a hen *, as they conſiſt of only two notes; whilſt the ſhort burſts of ſinging birds, contending with each other (called *jerks* by the bird-catchers), are equally diſtinguiſhed from what I term *ſong*, by their not continuing for four ſeconds.

As the notes of a cuckow and hen, therefore, though they exceed what I have defined the *call* of a bird to be, do not amount to its *ſong*, I will, for this reaſon, take the liberty of terming ſuch a ſucceſſion of two notes as we hear in theſe birds, the *varied call*.

Having thus ſettled the meaning of certain words, which I ſhall be obliged to make uſe of, I ſhall now proceed to ſtate ſome general principles with regard to the ſinging of birds, which ſeem to reſult from the experiments I have been making for ſeveral years, and under a great variety of circumſtances.

Notes in birds are no more innate, than language is in man, and depend entirely upon the maſter under which they are bred, as far as their organs will enable them to imitate the ſounds which they have frequent opportunities of hearing.

* The common hen, when ſhe lays, repeats the ſame note very often, and concludes with the ſixth above, which ſhe holds for a longer time.

Moſt

Moſt of the experiments I have made on this ſubject have been made with cock linnets, which were fledged and nearly able to leave their neſt, on account not only of this bird's docility, and great powers of imitation, but becauſe the cock is eaſily diſtinguiſhed from the hen at that early period, by the ſuperior whiteneſs in the wing *.

In many other ſorts of finging birds the male is not at the age of three weeks ſo certainly known from the female; and if the pupil turns out to be a hen,

" ——— ibi omnis
" Effuſus labor."

The Greek poets made a ſongſter of the τετ]ιξ, whatever animal that may be, and it is remarkable that they obſerved the female was incapable of ſinging as well as hen birds:

Ειτ' εισιν οι τετ]ιγες ȣκ ευδαιμονες,
Ων ταις γυναιξιν ȣ δ' οτιȣν φωνης ενι;
Comicorum Græcorum Sententiæ, p. 452.
Ed. Steph.

I have indeed known an inſtance or two of a hen's making out ſomething like the ſong of her ſpecies; but theſe are as rare as the common hen's being heard to crow.

I rather ſuſpect alſo, that thoſe parrots, magpies, &c. which either do not ſpeak at all, or very little, are hens of thoſe ſpecies.

* The white reaches almoſt to the ſhaft of the quill feathers, and in the hen does not exceed more than half.

I have educated neſtling linnets under the three beſt ſinging larks, the *ſkylark*, *woodlark*, and *titlark*, every one of which, inſtead of the linnet's ſong, adhered entirely to that of their reſpective inſtructors.

When the note of the *titlark-linnet* * was thoroughly *fixed*, I hung the bird in a room with two common linnets, for a quarter of a year, which were full in ſong; the *titlark-linnet*, however, did not borrow any paſſages from the linnet's ſong, but adhered ſtedfaſtly to that of the titlark.

I had ſome curioſity to find out whether an European neſtling would equally learn the note of an African bird: I therefore educated a young linnet under a *vengolina* †, which imitated its African maſter ſo exactly, without any mixture of the linnet ſong, that it was impoſſible to diſtinguiſh the one from the other.

This *vengolina-linnet* was abſolutely perfect, without ever uttering a ſingle note by which it could have been known to be a linnet. In ſome of my other experiments, however, the neſtling linnet retained the *call* of its own ſpecies, or what the bird-

* I thus call a bird which ſings notes he would not have learned in a wild ſtate; thus by a *ſkylark-linnet*, I mean a linnet with the ſkylark ſong; a *nightingale robin*, a robin with the nightingale ſong, &c.

† This bird ſeems not to have been deſcribed by any of the ornithologiſts; it is of the *finch* tribe, and about the ſame ſize with our aberdavine (or ſiſkin). The colours are grey and white, and the cock hath a bright yellow ſpot upon the rump. It is a very familiar bird, and ſings better than any of thoſe which are not European, except the American *mocking bird*.

catchers

catchers term the linnet's *chuckle*, from ſome reſemblance to that word when pronounced.

I have before ſtated, that all my neſtling linnets were three weeks old, when taken from the neſt; and by that time they frequently learn their *own call* from the parent birds, which I have mentioned to conſiſt of only a ſingle note.

To be certain, therefore, that a neſtling will not have even the *call* of its ſpecies, it ſhould be taken from the neſt when only a day or two old; becauſe, though neſtlings cannot ſee till the ſeventh day, yet they can hear from the inſtant they are hatched, and probably, from that circumſtance, attend to ſounds, more than they do afterwards, eſpecially as the call of the parents announces the arrival of their food.

I muſt own, that I am not equal myſelf, nor can I procure any perſon to take the trouble of breeding up a bird of this age, as the odds againſt its being reared are almoſt infinite. The warmth indeed of incubation may be, in ſome meaſure, ſupplied by cotton and fires; but theſe delicate animals require, in this ſtate, being fed almoſt perpetually, whilſt the nouriſhment they receive ſhould not only be prepared with great attention, but given in very ſmall portions at a time.

Though I muſt admit, therefore, that I have never reared myſelf a bird of ſo tender an age, yet I have happened to ſee both a linnet and a goldfinch which were taken from their neſts when only two or three days old.

The firſt of theſe belonged to Mr. Matthews, an apothecary at Kenſington, which, from a want of

other

other ſounds to imitate, almoſt articulated the words *pretty boy*, as well as ſome other ſhort ſentences: I heard the bird myſelf repeat the words *pretty boy*; and Mr. Matthews aſſured me, that he had neither the note or call of any bird whatſoever.

This talking linnet died laſt year, and many people went from London to hear him ſpeak.

The goldfinch I have before mentioned, was reared in the town of Knighton in Radnorſhire, which I happened to hear, as I was walking by the houſe where it was kept.

I thought indeed that a *wren* was ſinging; and I went into the houſe to inquire after it, as that little bird ſeldom lives long in a cage.

The people of the houſe, however, told me, that they had no bird but a goldfinch, which they conceived to ſing its own natural note, as they called it; upon which I ſtaid a conſiderable time in the room, whilſt its notes were merely thoſe of a *wren*, without the leaſt mixture of goldfinch.

On further inquiries, I found that the bird had been taken from the neſt when only two or three days old, that it was hung in a window which was oppoſite to a ſmall garden, whence the neſtling had undoubtedly acquired the notes of the wren, without having had any opportunity of learning even the *call* of the goldfinch.

Theſe facts which I have ſtated ſeem to prove very deciſively, that birds have not any innate ideas of the notes which are ſuppoſed to be peculiar to each ſpecies. But it will poſſibly be aſked, why in a wild ſtate they adhere ſo ſteadily to the ſame ſong,

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in ſo much that it is is well known, before the bird is heard, what notes you are to expect from him.

This, however, ariſes entirely from the neſtling's attending only to the inſtruction of the parent bird, whilſt it diſregards the notes of all others, which may perhaps be ſinging round him.

Young Canary-birds are frequently reared in a room where there are many other ſorts; and yet I have been informed that they only learn the ſong of the parent cock.

Every one knows, that the common houſe-ſparrow, when in a wild ſtate, never does any thing but chirp: this, however, does not ariſe from want of powers in this bird to imitate others; but becauſe he only attends to the parental note.

But, to prove this deciſively, I took a common Sparrow from the neſt when it was fledged, and educated him under a linnet: the bird, however, by accident heard a goldfinch alſo, and his ſong was, therefore, a mixture of the linnet and goldfinch.

I have tried ſeveral experiments, in order to obſerve from what circumſtances birds fix upon any particular note when taken from the parents; but cannot ſettle this with any ſort of preciſion, any more than at what period of their *recording* they determine upon the ſong to which they will adhere.

I educated a young robin under a very fine Nightingale; which, however, began already to be out of ſong, and was perfectly mute in leſs than a fortnight.

This robin afterwards ſung three parts in four *nightingale*; and the reſt of his ſong was what the

bird-

bird-catchers call *rubbiſh*, or no particular note whatſoever.

I hung this robin nearer to the nightingale than to any other bird; from which firſt experiment I conceived, that the ſcholar would imitate the maſter which was at the leaſt diſtance from him.

From ſeveral other experiments, however, which I have ſince tried, I find it to be very uncertain what notes the neſtling will moſt attend to, and often their ſong is a mixture; as in the inſtance which I before ſtated of the ſparrow.

I muſt own alſo, that I conceived, from the experiment of educating the robin under a nightingale, that the ſcholar would fix upon the note which it firſt heard when taken from the neſt; I imagined likewiſe, that, if the nightingale had been fully in ſong, the inſtruction for a fortnight would have been ſufficient.

I have, however, ſince tried the following experiment, which convinces me, ſo much depends upon circumſtances, and perhaps caprice in the ſcholar, that no general inference, or rule, can be laid down with regard to either of theſe ſuppoſitions.

I educated a neſtling robin under a woodlark-linnet, which was full in ſong, and hung very near to him for a month together: after which, the robin was removed to another houſe, where he could only hear a ſkylark-linnet. The conſequence was that the neſtling did not ſing a note of woodlark (though I afterwards hung him again juſt above the wood-lark-linnet) but adhered entirely to the ſong of the ſkylark-linnet.

Having

Having thus ſtated the reſult of ſeveral experiments, which were chiefly intended to determine, whether birds had any innate ideas of the notes, or ſong, which is ſuppoſed to be peculiar to each ſpecies, I ſhall now make ſome general obſervations on their ſinging; though perhaps the ſubject may appear to many a very minute one.

Every poet, indeed, ſpeaks with raptures of the harmony of the groves; yet thoſe even, who have good muſical ears, ſeem to pay little attention to it, but as a pleaſing noiſe.

I am alſo convinced (though it may ſeem rather paradoxical), that the inhabitants of London diſtinguiſh more accurately, and know more on this head, than of all the other parts of the iſland taken together.

This ſeems to ariſe from two cauſes.

The firſt is, that we have not more muſical ideas which are innate, than we have of language; and therefore thoſe even, who have the happineſs to have organs which are capable of receiving a gratification from this ſixth ſenſe (as it hath been called by ſome) require, however, the beſt inſtruction.

The orcheſtra of the opera, which is confined to the metropolis, hath diffuſed a good ſtile of playing over the other bands of the capital, which is, by degrees, communicated to the fidler and ballad-ſinger in the ſtreets; the organs in every church, as well as thoſe of the Savoyards, contribute likewiſe to this improvement of muſical faculties in the Londoners.

If the ſinging of the ploughman in the country is therefore compared with that of the London black-

guard, the ſuperiority is infinitely on the ſide of the latter; and the ſame may be obſerved in comparing the voice of a country girl and London houſe-maid, as it is very uncommon to hear the former ſing tolerably in tune.

I do not mean by this, to aſſert that the inhabitants of the country are not born with as good muſical organs; but only, that they have not the ſame opportunities of learning from others, who play in tune themſelves.

The other reaſon for the inhabitants of London judging better in relation to the ſong of birds, ariſes from their hearing each bird ſing diſtinctly, either in their own or their neighbours ſhops; as alſo from a bird continuing much longer in ſong whilſt in a cage, than when at liberty; the cauſe of which I ſhall endeavour hereafter to explain.

Thoſe who live in the country, on the other hand, do not hear birds ſing in their woods for above two months in the year, when the confuſion of notes prevents their attending to the ſong of any particular bird; nor does he continue long enough in a place, for the hearer to recollect his notes with accuracy.

Beſides this, birds in the ſpring ſing very loud indeed; but they only give ſhort jerks, and ſcarcely ever the whole compaſs of their ſong.

For theſe reaſons, I have never happened to meet with any perſon, who had not reſided in London, whoſe judgement or opinion on this ſubject I could the leaſt rely upon; and a ſtronger proof of this cannot be given, than that moſt people, who keep Ca-

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nary-birds do not know that they ſing chiefly either the titlark, or nightingale notes *.

Nothing, however, can be more marked than the note of a nightingale called its *jug*, which moſt of the Canary-birds brought from the Tyrol commonly have, as well as ſeveral nightingale *ſtrokes*, or particular paſſages in the ſong of that bird.

I mention this ſuperior knowledge in the inhabitants of the capital, becauſe I am convinced, that, if others are conſulted in relation to the ſinging of birds, they will only miſlead, inſtead of giving any material or uſeful information †.

Birds in a wild ſtate do not commonly ſing above ten weeks in the year; which is then alſo confined to the cocks of a few ſpecies; I conceive, that this laſt

* I once ſaw two of theſe birds which came from the Canary iſlands; neither of which had any ſong at all; and I have been informed, that a ſhip brought a great many of them not long ſince, which ſung as little.

Moſt of thoſe Canary-birds, which are imported from the Tyrol, have been educated by parents, the progenitor of which was inſtructed by a nightingale; our Engliſh Canary-birds have commonly more of the titlark note.

The traffick in theſe birds makes a ſmall article of commerce, as four Tyroleze generally bring over to England ſixteen hundred every year; and though they carry them on their backs one thouſand miles, as well as pay 20l. duty for ſuch a number, yet upon the whole it anſwers to ſell theſe birds at 5s. a piece.

The chief place for breeding Canary-birds is Inſpruck and its environs, from whence they are ſent to Conſtantinople, as well as every part of Europe.

† As it will not anſwer to catch birds with clap-nets any where but in the neighbourhood of London, moſt of the birds which may be heard in a country town are neſtlings, and conſequently cannot ſing the ſuppoſed natural ſong in any perfection.

circumſtance ariſes from the ſuperior ſtrength of the muſcles of the larynx.

I procured a cock nightingale, a cock and hen blackbird, a cock and hen rook, a cock linnet, as alſo a cock and hen chaffinch, which that very eminent anatomiſt, Mr. Hunter, F. R. S. was ſo obliging as to diſſect for me, and begged, that he would particularly attend to the ſtate of the organs in the different birds, which might be ſuppoſed to contribute to ſinging.

Mr. Hunter found the muſcles of the larynx to be ſtronger in the nightingale than in any other bird of the ſame ſize; and in all thoſe inſtances (where he diſſected both cock and hen) that the ſame muſcles were ſtronger in the cock.

I ſent the cock and hen rook, in order to ſee whether there would be the ſame difference in the cock and hen of a ſpecies which did not ſing at all. Mr. Hunter, however, told me, that he had not attended ſo much to their comparative organs of voice, as in the other kinds; but that, to the beſt of his recollection, there was no difference at all.

Strength, however, in theſe muſcles, ſeems not to be the only requiſite; the birds muſt have alſo great plenty of food, which ſeems to be proved ſufficiently by birds in a cage ſinging the greateſt part of the year, when the wild ones do not (as I obſerved before) continue in ſong above ten weeks.

The food of ſinging birds conſiſts of plants, inſects, or ſeeds, and of the two firſt of theſe there is infinitely the greateſt profuſion in the ſpring.

As for ſeeds, which are to be met with only in the autumn, I think they cannot well find any great quantities

quantities of them in a country ſo cultivated as England is; for the ſeeds in meadows are deſtroyed by mowing; in paſtures, by the bite of the cattle; and in arable, by the plough, when moſt of them are buried too deep for the bird to reach them *.

I know well that the ſinging of the cock-bird in the ſpring is attributed by many † to the motive only of pleaſing its mate during incubation.

Thoſe, however, who ſuppoſe this, ſhould recollect, that much the greater part of birds do not ſing at all: why ſhould their mate therefore be deprived of this ſolace and amuſement?

The bird in a cage, which, perhaps, ſings nine or ten months in a year, cannot do ſo from this inducement; and, on the contrary, it ariſes chiefly from contending with another bird, or indeed againſt almoſt any ſort of continued noiſe.

Superiority in ſong gives to birds a moſt amazing aſcendency over each other; as is well known to the bird-catchers by the faſcinating power of their call-birds, which they contrive ſhould moult prematurely for this purpoſe.

But, to ſhew deciſively that the ſinging of a bird in the ſpring does not ariſe from any attention to its mate, a very experienced catcher of nightingales hath informed me, that ſome of theſe birds have *jerked* the inſtant they were caught. He hath alſo brought

* The plough indeed may turn up ſome few ſeeds, which may ſtill be in an eatable ſtate.

† See, amongſt others, M. de Buffon, in his lately-publiſhed Ornithology.

to

to me a nightingale, which had been but a few hours in a cage, and which burſt forth in a roar of ſong.

At the ſame time this bird is ſo ſulky on its firſt confinement, that he muſt be crammed for ſeven or eight days, as he will otherwiſe not feed himſelf: it is alſo neceſſary to tye his wings, to prevent his killing himſelf againſt the top or ſides of the cage.

I believe there is no inſtance of any bird's ſinging which exceeds our blackbird in ſize; and poſſibly this may ariſe from the difficulty of its concealing itſelf, if it called the attention of its enemies, not only by bulk, but by the proportionable loudneſs of its notes *.

I ſhould rather conceive, it is for the ſame reaſon that no hen-bird ſings, becauſe this talent would be ſtill more dangerous during incubation; which may poſſibly alſo account for the inferiority in point of plumage.

I ſhall now conſider how far the ſinging of birds reſembles our known muſical intervals, which are never marked more minutely than to half notes; becauſe, though we can form every gradation from half-note to half-note, by drawing the finger gently over the ſtring of a violin, or covering by degrees the hole of a flute; yet we cannot produce ſuch a minute interval at command, when a quarter-note for example might be required.

Ligon, indeed, in his hiſtory of Barbadoes, hath the following paſſage: " The next bird is of the co-

* For the ſame reaſon, moſt large birds are wilder than the ſmaller ones.

" lour

" lour of the fieldfare; but the head is too large " for the body; and for that reaſon ſhe is called a " counſellor. She performs that with her voice, " which no inſtrument can play, or voice can ſing; " and that is quarter-notes, her ſong being compoſed " of them, and every one a note higher than another." Hiſt. Barb. p. 60.

Ligon appears, from other parts of his work, to have been muſical; but I ſhould doubt much whether he was quite ſure of theſe quarter intervals, ſo as to ſpeak of them with preciſion.

Some paſſages of the ſong in a few kinds of birds correſpond with the intervals of our muſical ſcale (of which the cuckow is a ſtriking and known inſtance): much the greater part, however, of ſuch ſong is not capable of muſical notations.

This ariſes from three cauſes: the firſt is, that the rapidity is often ſo great, and it is alſo ſo uncertain when they may ſtop, that we cannot reduce the paſſages to form a muſical bar, in any time whatſoever.

The ſecond is, that the pitch of moſt birds is conſiderably higher * than the moſt ſhrill notes of thoſe inſtruments which contain even the greateſt compaſs.

* Dr. Wallis is miſtaken in part of what he ſuppoſes to be the cauſe of ſhrillneſs in the voice, " Nam ut tubus, ſic trachea " longior, & ſtrictior, ſonum efficit magis acutum." Grammar, p. 3.

The narrower the pipe is, the more ſharp the pitch as he rightly obſerves; but the length of the tube hath juſt the contrary effect, becauſe players on the flute always inſert a longer middle piece, when they want to make the inſtrument more flat.

I have

I have before ſaid, that our ideas of a voice, or inſtrument, being perfectly in tune or not, ariſe from comparing it with the muſical intervals to which we are moſt accuſtomed.

As the upper and lower parts of every inſtrument, however, are but ſeldom uſed, we are not ſo well acquainted with the intervals in the higheſt and loweſt octaves, as we are with thoſe which are more central; and for this reaſon the harpſichord-tuners find it more difficult to tune theſe extreme parts.

As a bird's pitch, therefore, is higher than that of any inſtrument, we are conſequently at a ſtill greater loſs when we attempt to mark their notes in muſical characters, which we can ſo readily apply to ſuch as we can diſtinguiſh with preciſion.

The third, however, and unſurmountable difficulty is, that the intervals uſed by birds are commonly ſo minute, that we cannot judge at all of them from the more groſs intervals into which we divide our muſical octave.

It ſhould therefore be recollected, by thoſe who have contended that the Greeks and Romans were acquainted with ſuch more minute intervals of the octave, that they muſt inſiſt the ancients had organs of ſenſation, with which their degenerate poſterity are totally unprovided.

Though we cannot attain the more delicate and imperceptible intervals in the ſong of birds *, yet many of them are capable of whiſtling tunes with our more groſs intervals, as is well known by the

* There have been inſtances indeed of perſons who would whiſtle the notes of birds, but theſe are too rare to be argued from.

common

common inſtances of piping Bullfinches *, and Canary-birds.

This, however, ariſes from mere imitation of what they hear when taken early from the neſt; for if the inſtrument from which they learn is out of tune, they as readily pipe the falſe, as the true notes of the compoſition.

The next point of compariſon to be made between our muſic and that of birds is, whether they always ſing in the ſame pitch.

This, however, I will not preſume to anſwer with any preciſion, for the reaſon I have before ſuggeſted; I ſhall, however, without reſerve, give the beſt conjectures I can form on this head.

If a dozen ſinging birds of different kinds are heard in the ſame room, there is not any diſagreeable diſſonance (which is not properly reſolved), either to my own ear, or to that of others, whoſe judgement on ſuch a point I can more rely.

At the ſame time, as each bird is ſinging a different ſong, it is extraordinary that what we call harmony ſhould not be perpetually violated, as we experience, in what is commonly called a Dutch concert, when ſeveral tunes are played together.

The firſt requiſite to make ſuch ſounds agreeable to the ear is, that all the birds ſhould ſing in the ſame key, which I am induced to believe that they do, from the following reaſons.

I have long attended to the ſinging of birds, but if I cannot have recourſe to an inſtrument very ſoon,

* Theſe Bullfinches alſo form a ſmall article of commerce, and are chiefly brought from the neighbourhood of Cologne.

I cannot carry the pitch of their notes in my memory, even for a very ſhort time.

I therefore deſired a very experienced harpſichord-tuner (who told me he could recollect any particular note which he happened to hear for ſeveral hours), to mark down when he returned home what he had obſerved on this head.

I have lately received an account from him of the following notes in different birds.

F. natural in woodlarks.
A. natural in common cocks.
C. natural in Bantam cocks.
B. flat in a very large cock.
C. falling to A. commonly in the cuckow.
A. in thruſhes.
D. in ſome owls.
B. flat in ſome others.

Theſe obſervations furniſh five notes, viz. A B. flat, C. D. and F. to which I can add a ſixth, (viz. G.) from my own obſervations on a nightingale which lived three years in a cage. I can alſo confirm theſe remarks of the harpſichord-tuner by having frequently heard from the ſame bird C. and F.

As one ſhould ſpeak of the pitch of theſe notes with ſome preciſion, the B. flat of the ſpinnet I tried them by, was perfectly in tune with the great bell of St. Paul's.

The following notes, therefore, having been obſerved in different birds, viz. A. B. flat, C. D. F. and G. the E. is only wanting to compleat the ſcale; the ſix other notes, however, afford ſufficient data for making ſome conjectures, at leaſt, with regard to the key in which birds may be ſuppoſed to ſing,

as

as theſe intervals can only be found in the key of F. with a ſharp third, or that of G. with a flat third.

I muſt own, I ſhould rather ſuppoſe it to be the latter, and for the following reaſons.

Lucretius ſays (and perhaps the conjecture is not only ingenious but well founded), that the firſt muſical notes were learned from birds:

" At liquidas avium voces imitarier ore
" Ante fuit multo, quam lævia carmina cantu
" Concelebrare homines poſſent, cantuque juvare."

Now, of all the muſical tones which can be diſtinguiſhed in birds, thoſe of the cuckow have been moſt attended to, which form a flat third, not only by the obſervations of the harpſichord tuner I have before mentioned, but likewiſe by thoſe of Kircher, in his *Muſurgia*.

I know well that there have been ſome late compoſitions, which introduce the cuckow notes in a ſharp third; theſe compoſers, however, did not trouble themſelves with accuracy in imitating theſe notes, and it anſwered their purpoſe ſufficiently, if there was a general reſemblance.

Another proof of our muſical intervals being originally borrowed from the ſong of birds, ariſes from moſt compoſitions being in a flat third, where muſic is ſimple, and conſiſts merely of melody.

The oldeſt tune I happen to have heard is a Welſh one, called *Morvar Rhydland* *, which is

* Or *Rhydland Marſh*, where the Welſh received a great defeat; Rhydland is in Flintſhire. We find alſo, by the *Orpheus Britannicus*, that even ſo late as the time of Purcel, two parts in the three of his compoſitions are in the flat third.

compoſed in a flat third; and if the muſic of the Turks and Chineſe is examined in Du Halde and Dr. Shaw, half of the airs are alſo in a flat third.

The muſic of two centuries ago is likewiſe often in a flat third, though ninety-nine compoſitions out of a hundred are now in the ſharp third.

The reaſon, however, of this alteration ſeems to be very clear: the flat third is plaintive, and conſequently adapted to ſimple movements, ſuch as may be expected in countries where muſic hath not been long cultivated.

There is on the other hand a moſt ſtriking brilliancy in the ſharp third, which is therefore proper for the amazing improvements in execution, which both ſingers and players have arrived at within the laſt fifty years.

When Corelli's muſic was firſt publiſhed, our ableſt violiniſts conceived that it was too difficult to be performed; it is now, however, the firſt compoſition which is attempted by a ſcholar. Every year alſo now produces greater and greater prodigies upon other inſtruments, in point of execution.

I have before obſerved, that by attending to a nightingale, as well as a robin which was educated under him, I always found that the notes reducible to our intervals of the octave were preciſely the ſame; which is another proof that birds ſing always in the ſame key.

In this circumſtance, they differ much from the human ſinger; becauſe thoſe who are not able to *ſing at ſight*, often begin a ſong either above or below the compaſs of their voice, which they are not therefore able to go through with. As birds, however, form the ſame paſſages with the ſame notes,

Allegretto

1st. B.

2d. B.

1st. B.

2d. B.

Allegro

1st. B.

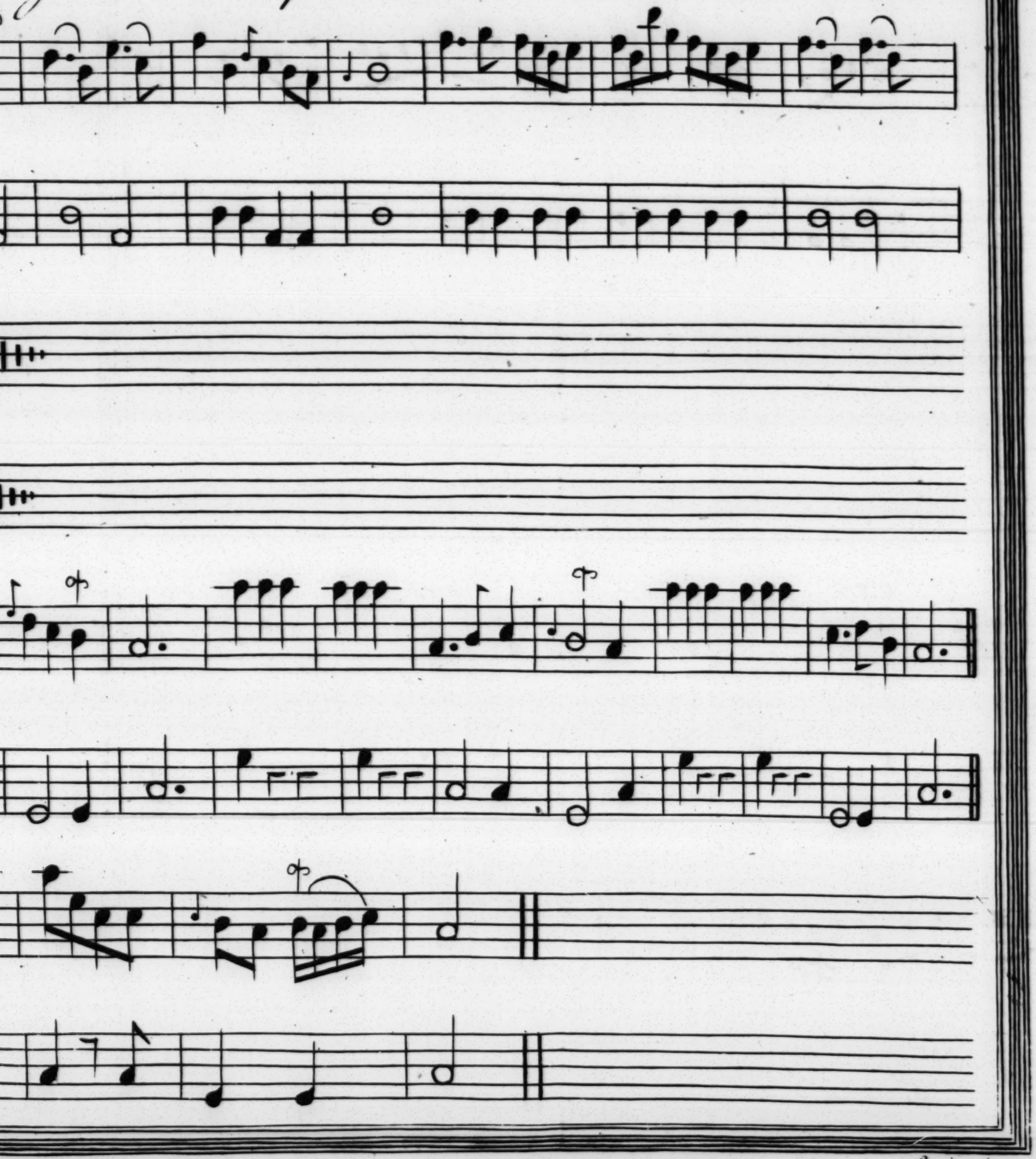
2d. B.

Allegro

1st. B.

2d. B.

ing) Bullfinches.



Allegretto

1st. B.

2d. B.

1st. B.

2d. B.

Allegro

1st. B.

2d. B.

Allegro

1st. B.

2d. B.

at all times, this miſtake of the pitch can never happen in them.

Few ſingers again can continue their own part, whilſt the ſame paſſages are ſung by another in a different key; or if the ſame or other paſſages are ſung, ſo as not to coincide with the muſical bar, or time of the firſt ſinger.

As birds however adhere ſo ſtedfaſtly to the ſame preciſe notes in the ſame paſſages, though they never trouble themſelves about what is called *time* in muſic; it follows that a compoſition may be formed for two piping bulfinches, in two parts, ſo as to conſtitute true harmony, though either of the birds may happen to begin, or ſtop, when they pleaſe.

I have therefore procured ſuch an ingenious compoſition, by a very able muſician *, which I ſend herewith; and it need ſcarcely be obſerved, that there cannot poſſibly be much variety in the part of the ſecond bulfinch. [See TAB. XI.]

Though ſeveral birds have great muſical powers, yet they ſeem to have no delicacy of ſenſations, as the human ſinger hath; and therefore the very beſt of them cannot be taught to exceed the inſipidity of the upper part of the flute ſtop of an organ †, which hath not the modern improvement of a *ſwell*.

* Mr. Zeidler, who plays the violincello at Covent Garden theatre.

† Lord Bacon mentions, that in the inſtrument called a *regall* (which was a ſpecies of portable organ), there was a *nightingale* ſtop, in which water was made uſe of to produce the ſtronger imitation of this bird's tone. See Cent. ii. exper. 172. Though this inſtrument, as well as its *Nightingale* ſtop, is now diſuſed, I have procured an organ pipe to be immerſed partly in water, which, when blown into, hath produced a tone very ſimilar to that of birds.

They

They are alſo eaſily impoſed upon by that moſt imperfect of all inſtruments, a *bird-call*, which they often miſtake for the notes of their own ſpecies.

I have before obſerved, that perhaps no bird may be ſaid to ſing which is larger than a blackbird, though many of them are taught to ſpeak: the ſmaller birds, however, have this power of imitation; though perhaps the larger ones have not organs which may enable them, on the other hand, to ſing.

We have the following inſtances of birds being taught to ſpeak, in the time of the Greeks and Romans, upon which we never try the ſame experiment. Moſchus addreſſes nightingales and ſwallows which were thus inſtructed:

Αδονιδες, ωασαι τε χελιδονες, ας ωοκ' ελερπεν,
Ας λαλειν εδιδασκε.

Moſchi Idyl. iii.

Pliny mentions both a cock, thruſh, and nightingales, which articulated *:

" Habebant & Cæſares juvenes turdum †, item
" *luſcinias* Græco atque Latino ſermone dociles, præ-
" terea meditantes in diem, & aſſidue nova lo-
" quentes longiore etiam contextu."

Statius alſo takes notice of ſome birds ſpeaking, which we never attempt to teach in this manner:

* Lib. x. c. 21 & 42.

† Ibid. The other turdus belonged to the Empreſs Agrippina.

" Huc

" Huc doctæ ſtipentur aves, queis nobile fandi
" Jus natura dedit, plangat Phœbeius ales,
" Auditaſque memor penitus demittere voces
" Sturnus, & Aonio verſæ certamine picæ;
" Quique refert jungens iterata vocabula perdix,
" Et quæ Biſtonio queritur ſoror orba cubili *."
Stat. Sylv. lib. ii. ecl. 4.

* Amongſt the five birds mentioned in theſe lines of Statius, there are four which are never taught to ſpeak at preſent, viz. the cock, the nightingale, the common, and the *red legged partridge*.

As I ſuppoſe, however, that *perdix* ſignifies this laſt bird, and not the common partridge (as it is always tranſlated), it is proper I ſhould here give my reaſons why I diſſent from others, as alſo why I conceive that *ſturnus*, in this paſſage, is not a *ſtarling*, but the common partridge.

None of the ancients have deſcribed the plumage of the *perdix*; but Ariſtotle, Ovid, and Pliny, inform us of what materials the neſt of this bird is compoſed, as well as where it is placed.

Ariſtotle ſays, that the neſt is *fortified with wood* [a]; and in another chapter [b], with *thorns and wood*; neither of which are uſed by the common partridge, which often builds in a country where they cannot be procured.

On the contrary, M. de Buffon informs us, that the red legged partridge, " ſe tiennent ſur les montagnes qui produiſent " beaucoup de bruyeres, & de broſſailles [c].

[a] Επηλυγαζομεναι υλην. Lib. v. c. 1. Which Stephens renders *making a covering of wood*.

[b] Lib. ix. c. 8. The common partridge, however, makes its neſt with hay and ſtraw.

[c] Orn. T. II. p. 433.

Silius Italicus alſo deſcribes the *pernix* as being found in the ſame ſort of country,

" Ceu *pernix*, quam denſa vagis latratibus implet
" Venator *dumeta* Iacon." Lib. iii. l. 29.

As

As we find, from theſe citations, that ſo many different ſorts of birds have learned to ſpeak, and as I have ſhewn that a ſparrow may be taught to

Ovid, therefore, ſpeaking of the *perdix*, ſays,

" —— ponitque in ſepibus ova [d],"

where the common partridge is ſeldom known to build.

Pliny again informs us, " perdices ſpinâ & frutice ſic mu- " niunt receptaculum, ut contra feras abunde valentur [e]", as alſo in the 52d chapter of his tenth book, that the *perdix* lays white eggs, which is not true of the common partridge.

But there are not wanting other proofs of the conjecture I have here made.

Ariſtotle, ſpeaking of this ſame bird, ſays, Των μεν περδικων, οι μεν καττυβιζουσιν, οι δε πριουσι [f].

Now, the word καττυβιζουσι is clearly formed from the *call* of the bird alluded to, which does not at all reſemble that of the common partridge.

Thus alſo the author of the Elegy on the Nightingale, who is ſuppoſed by ſome to be Ovid, hath the following line:

" *Caccabat* hinc perdix, hinc gratitat improbus anſer."

ſo that the call of the bird muſt have had ſomething very particular, and have anſwered nearly, to the words καττυβιζει and *caccabat*.

I find, indeed, that M. de Buffon contends [g] that the περδιξ of Ariſtotle does not mean the common partridge, but the bartavel, with regard to which, I ſhall not enter into any diſcuſſion, but only obſerve, that moſt of his references are inaccurate, and that he entirely miſtakes the materials of which the neſt is compoſed, according to Ariſtotle's ſixth book, and firſt chapter.

[d] Ovid. Met. lib. viii. l. 258. I ſhall alſo refer to l. 237, of the ſame book:

" Garrula ramoſâ proſpexit ab ilice perdix:"

as it is well known that the common partridge never perches upon a tree.

[e] Lib. x. c. 23. [f] Lib. iv. c. 9. [g] Orn. T. II. p. 422.

ſing

ſing the linnet's note, I ſcarcely know what ſpecies to fix upon, that may be conſidered as incapable of ſuch imitations; for it is very clear, from ſeveral experiments before ſtated, that the utmoſt endeavours will not be wanting in the bird, if he is endowed with the proper organs.

It can therefore only be ſettled by educating a bird, under proper circumſtances, whether he is thus qualified or not; for if one was only to determine this point by conjecture, one ſhould ſuppoſe

If, indeed, M. de Buffon had not been thus inaccurate, the graſs and leaves, of which he ſays it is made, would prove that Ariſtotle ſpeaks of the common partridge, contrary to what he himſelf ſuppoſes.

But the ſtrongeſt proof that *perdix* ſignifies the red legged partridge is, that the Italians to this day call this bird *pernice* [h], and the common ſort *ſtarna* [i].

This alſo now brings me to the proofs, of *ſturnus* in this paſſage of Statius ſignifying the *common partridge, and not the ſtarling*, which I muſt admit are not ſo ſtrong as with regard to the import of the word *perdix*. If my arguments are not therefore ſo convincing on this head, the number of birds taught to ſpeak by the Romans, and not by us, muſt be reduced to three, as the ſtarling is frequently learned to talk in the preſent times.

As I cannot argue from the deſcription of the habits of the *ſturnus*, or the materials of its neſt, as in the former inſtance, I muſt reſt my conjecture (ſuch as it is) on the two birds, almoſt following each other in theſe lines of Statius; on the common partridge being called *ſtarna* to this day by the Italians, and upon the Romans having had otherwiſe no name for our partridge (which is a very common bird in Italy), if *ſturnus* is ſuppoſed to ſignify only a *ſtarling*.

[h] I cannot therefore but think that *pernix*, according to the reading in the paſſage of Silius Italicus, which I have before cited, is the true Latin name for this bird, and that it was called *perdix* by thoſe only who underſtood Greek.

[i] See Olina.

that a ſparrow would not imitate the ſong of the linnet, nor that a nightingale or partridge could be taught to ſpeak.

And here it may not be improper to explain what I mean by birds learning to imitate the notes of others, or the human ſpeech.

If the birds differ little in ſhape or ſize (particularly of the beak *), the imitation is commonly ſo ſtrong, that

* It ſeems very obvious why the form and ſize of the beak may be material; but I have alſo obſerved, that the colour of a bird's bill changes, when in or out of ſong; and I am informed that a cock ſeldom crows much, but when his comb is red.

When moſt of the finch tribe are coming into ſong, there is ſuch a gradual change in the colour of their bill; thus, thoſe of the chaffinch and linnet are then of a very deep blue, which fades away again, when the bird ceaſes to be in ſong.

This particular ſhould be attended to by the ornithologiſt, in his deſcription; becauſe otherwiſe he ſuppoſes the colour of the bill to be permanent, which is by no means ſo.

This alteration, however, rather ſeems to be the ſymptom than the cauſe of a bird's coming into ſong, or otherwiſe, and I have never attended to this circumſtance in the ſoft billed birds ſufficiently, to ſay whether it holds alſo with regard to them.

A very intelligent bird-catcher, however, was able to prognoſticate, for three winters together, when a nightingale, which I kept ſo long, was coming into ſong (though there was no change in the colour of the bill), by the dung's being intermixed with large bloody ſpots, which before was only of a dead white.

This ſame bird-catcher was alſo very ſucceſsful in his preſcriptions for ſick birds, with regard to the ingredients of which he was indeed very myſterious.

He ſaid, that as he could not feel their pulſe, the circumſtances which he chiefly attended to were their weight, as well as both the conſiſtence and colour of their dung.

He always frankly ſaid what he expected from his preſcriptions, and that if ſuch and ſuch changes did not ſoon take place,

" Mine

" Mirè ſagaces falleret hoſpites
" Diſcrimen obſcurum." Horat.

for, in ſuch inſtances, the paſſages are not only the ſame, but the tone.

Such was the event of the experiment I have before mentioned of the linnet educated under a vengolina.

In my experiment, however, of teaching the ſparrow the notes of the linnet, though the ſcholar imitated the paſſages of its maſter, yet the tone of the ſparrow had by no means the mellowneſs of the original.

The imitation might therefore be, in ſome meaſure, compared to the ſinging of an opera ſong by a black-guard, when, though the notes may be preciſely the ſame, yet the manner and tone would differ very much.

Thus alſo the linnet, which I heard repeat the words *pretty boy*, did not articulate like a *parrot* or a *mino*, though, at the ſame time, the words might be clearly diſtinguiſhed.

The education I have therefore been ſpeaking of will not give new organs of voice to a bird, and the inſtrument itſelf will not vary, though the notes or paſſages may be altered almoſt at pleaſure.

I tried once an experiment, which might indeed have poſſibly made ſome alteration in the tone of a bird, from what it might have been when the animal

the caſe was deſperate. He frequently alſo refuſed to preſcribe, if the bird felt too light in the hand, or he thought that there was not ſufficient time to bring about an alteration in the dung.

was at its full growth, by procuring an operator who caponiſed a young blackbird of about ſix weeks old; as it died, however, ſoon afterwards, and I have never repeated the experiment, I can only conjecture with regard to what might have been the conſequences of it.

Both Pliny * and the London poulterers agree that a capon does not crow, which I ſhould conceive to ariſe from the muſcles of the larynx never acquiring the proper degree of ſtrength, which ſeems to be requiſite to the ſinging of a bird, from Mr. Hunter's diſſections.

But it will perhaps be aſked, why this operation ſhould not improve the notes of a neſtling, as much as it is ſuppoſed to contribute to the greater perfection of the human voice.

To this I anſwer, that caſtration by no means inſures any ſuch conſequence; for the voices of much the greater part of Italian eunuchs are ſo indifferent, that they have no means of procuring a livelihood but by copying muſic, and this is one of the reaſons why ſo few compoſitions are publiſhed in Italy, as it would ſtarve this refuſe of ſociety.

But it may be ſaid, that there hath been a Farinelli and a Manzoli, whoſe voices were ſo diſtinguiſhedly ſuperior.

To this I again anſwer, that the catalogue of ſuch names would be a very ſhort one; and that we attribute thoſe effects to caſtration, which ſhould rather be aſcribed to the education of theſe ſingers.

* Lib. x. c. 21.

Caſtration

Caſtration commonly leaves the human voice at the ſame pitch as when the operation is performed; but the eunuch, from that time, is educated with a view only to his future appearance on the opera ſtage; he therefore manages his voice to greater advantage, than thoſe who have not ſo early and conſtant inſtruction.

Conſidering the ſize of many ſinging birds, it is rather amazing at what a diſtance their notes may be heard.

I think I may venture to ſay, that a nightingale may be very clearly diſtinguiſhed at more than half a mile *, if the evening is calm. I have alſo obſerved the breath of a robin (which exerted itſelf) ſo condenſed in a froſty morning, as to be very viſible.

To make the compariſon, however, with accuracy, between the loudneſs of a bird's and the human voice, a perſon ſhould be ſent to the ſpot from whence the bird is heard; I ſhould rather conceive that, upon ſuch trial, the nightingale would be diſtinguiſhed further than the man.

It muſt have ſtruck every one, that, in paſſing under a houſe where the windows are ſhut, the ſinging of a bird is eaſily heard, when, at the ſame time, a converſation cannot be ſo, though an animated one.

Moſt people, who have not attended to the notes of birds, ſuppoſe that thoſe of every ſpecies ſing

* Monſ. de Buffon ſays, that the quadruped which he terms the *huarine*, may be heard at the diſtance of a league. Ornith. Tom. I.

exactly

exactly the ſame notes and paſſages, which is by no means true, though it is admitted that there is a general reſemblance.

Thus the London bird-catchers prefer the ſong of the Kentiſh goldfinches, but Eſſex chaffinches; and when they ſell the bird to thoſe who can thus diſtinguiſh, inform the buyer that it hath ſuch a note, which is very well underſtood between them *.

Some of the nightingale fanciers alſo prefer a Surry bird to thoſe of Middleſex †.

Theſe differences in the ſong of birds of the ſame ſpecies cannot perhaps be compared to any thing more appoſite, than the varieties of provincial dialects.

The nightingale ſeems to have been fixed upon, almoſt univerſally, as the moſt capital of ſinging birds, which ſuperiority it certainly may boldly challenge: one reaſon, however, of this bird's being

* Theſe are the names which they give to ſome of the nightingale's notes: *Sweet, Sweet jug, Jug ſweet, Water bubble, Pipe rattle, Bell pipe, Scroty, Skeg, Skeg, ſkeg, Swat ſwat ſwaty, Whitlow whitlow whitlow*, from ſome diſtant affinity to ſuch words.

† Mr. Henſhaw informs us, that nightingales in Denmark are not heard till May, and that their notes are not ſo ſweet or various as with' us. Dr. Birch's Hiſtory of the Royal Society, Vol. III. p. 189. Whilſt Mr. Fletcher (who was miniſter from Q. Elizabeth to Ruſſia) ſays, that the nightingales in that part of the world have a finer note than ours. See Fletcher's Life, in the Biographia Britannica.

I never could believe what is commonly aſſerted, that the Czar Peter was at a conſiderable expence to introduce ſinging birds near Peterſburgh; becauſe it appears, by the *Fauna Suecica*, that they have in thoſe latitudes moſt of the ſame birds with thoſe of England.

more attended to than others is, that it ſings in the night *.

Hence Shakeſpeare ſays,

" The nightingale, if ſhe ſhould ſing by day,
" When every gooſe is cackling, would be thought
" No better a muſician than the wren."

The ſong of this bird hath been deſcribed, and expatiated upon, by ſeveral writers, particularly Pliny and Strada.

As I muſt own, however, that I cannot affix any preciſe ideas to either of theſe celebrated deſcriptions, and as I once kept a very fine bird of this ſort for three years, with very particular attention to its ſong; I ſhall endeavour to do it the beſt juſtice I am capable of.

In the firſt place, its tone is infinitely more mellow than that of any other bird, though, at the ſame time, by a proper exertion of its muſical powers, it can be exceſſively brilliant.

When this bird *ſang its ſong round*, in its whole compaſs, I have obſerved ſixteen different beginnings and cloſes, at the ſame time that the intermediate notes were commonly varied in their ſucceſſion with ſuch judgment, as to produce a moſt pleaſing variety.

The bird which approaches neareſt to the excellence of the nightingale, in this reſpect, is the ſky-lark; but then the tone is infinitely inferior in point of mellowneſs: moſt other ſinging birds have not above four or five changes.

* The woodlark and reedſparrow ſing likewiſe in the night; and from hence, in the neighbourhood of Shrewſbury, the latter hath obtained the name of the willow-nightingale. Nightingales, however, and theſe two other birds, ſing alſo in the day, but are not then diſtinguiſhed in the general concert.

The next point of ſuperiority in a nightingale is its continuance of ſong, without a pauſe, which I have obſerved ſometimes not to be leſs than twenty ſeconds. Whenever reſpiration, however, became neceſſary, it was taken with as much judgment as by an opera ſinger.

The ſkylark again, in this particular, is only ſecond to the nightingale *.

* I ſhall here inſert a table, by which the comparative merit of the Britiſh ſinging birds may be examined, the idea of which I have borrowed from Monſ. de Piles, in his *Cours de Peinture par Principes*. I ſhall not be ſurprized, however, if, as he ſuggeſts, many may diſagree with me about particular birds, as he ſuppoſes they will do with him, concerning the merits of painters.

As I have five columns inſtead of the four which M. de Piles uſes, I make 20 the point of abſolute perfection, inſtead of 16, which is his ſtandard.

	Mellowneſs of tone.	Sprightly notes.	Plaintive notes.	Compaſs.	Execution.
Nightingale	19	14	19	19	19
Skylark	4	19	4	18	18
Woodlark	18	4	17	12	8
Titlark	12	12	12	12	12
Linnet	12	16	12	16	18
Goldfinch	4	19	4	12	12
Chaffinch	4	12	4	8	8
Greenfinch	4	4	4	4	6
Hedge-ſparrow	6	0	6	4	4
Aberdavine (or Siſkin)	2	4	0	4	4
Redpoll	0	4	0	4	4
Thruſh	4	4	4	4	4
Blackbird	4	4	0	2	2
Robin	6	16	12	12	12
Wren	0	12	0	4	4
Reed-ſparrow	0	4	0	2	2
Black-cap, or the Norfolk Mock nightingale [k]	14	12	12	14	14

[k] Brit. Zool. p. 262.

And

And here I muſt again repeat, that what I deſcribe is from a caged nightingale, becauſe thoſe which we hear in the ſpring are ſo rank, that they ſeldom ſing any thing but ſhort and loud jerks, which conſequently cannot be compared to the notes of a caged bird, as the inſtrument is overſtrained.

I muſt alſo here obſerve, that my Nightingale was a very capital bird; for ſome of them are ſo vaſtly inferior, that the bird-fanciers will not keep them, branding them with the name of Frenchmen *.

But it is not only in tone and variety that the Nightingale excells; the bird alſo ſings (if I may ſo expreſs myſelf) with ſuperior judgement and taſte.

I have therefore commonly obſerved, that my Nightingale began ſoftly like the ancient orators;

I have made no mention of the bulfinch in this table, which is commonly conſidered as a ſinging bird; becauſe its wild note, without inſtruction, is a moſt jarring and diſagreeable noiſe.

I have likewiſe omitted [l] the redſtart (which is called by the French le Roſſignol de Muraille), as I am not ſufficiently acquainted with its ſong, though it is admired by many; I ſhould rather conceive, however, with Zinanni, that there is no very extraordinary merit in the notes.

The London bird-catchers alſo ſell ſometimes the yellow hammer, twite and brambling [m] as ſinging birds; but none of theſe will come within my definition of what may be deemed ſo.

* One ſhould ſuppoſe from this, that the nightingale-catchers had heard much of the French muſic; which is poſſibly the caſe, as ſome of them live in Spittal-fields.

[l] Il culo ranzo é un ucello, (per quanto dicono) molto canoro, ma io tale non lo ſtimo. Delle uova é del nidi, p. 53.

[m] They call this bird a kate.

reſerving its breath to ſwell certain notes, which by this means had a moſt aſtoniſhing effect, and which eludes all verbal deſcription.

I have indeed taken down certain paſſages which may be reduced to our muſical intervals; but though by theſe means one may form an idea of ſome of the notes uſed, yet it is impoſſible to give their comparative durations in point of muſical tune, upon which the whole effect muſt depend.

I once procured a very capital player on the flute to execute the notes which Kircher hath engraved in his *Muſurgia*, as being uſed by the nightingale; when, from want of not being able to ſettle their comparative duration, it was impoſſible to obſerve any traces almoſt of the nightingale's ſong.

It may not be improper here to conſider, whether the nightingale may not have a very formidable competitor in the American mocking-bird *; though almoſt all travellers agree, that the concert in the European woods is ſuperior to that of the other parts of the globe †.

As birds are now annually imported in great numbers from Aſia, Africa, and America, I have frequently attended to their notes, both ſingly and in concert, which are certainly not to be compared to thoſe of Europe.

Thomſon, the poet, (whoſe obſervations in natural hiſtory are much to be depended upon) makes

* Turdus Americanus minor canorus. Ray's Syn. It is called by the Indians Contlatolli; which is ſaid to ſignify four hundred tongues. See alſo Cateſby.

† See Rocheſort's Hiſt des Antelles, T. I. p. 366.—Ph. Tr. Abr. Vo. III. p. 563.—andCateſby.

this

this ſuperiority in the European birds to be a ſort of compenſation for their great inferiority in point of gaudy plumage. Our goldfinch, however, joins to a very brilliant and pleaſing ſong, a moſt beautiful variety of colours in its feathers *.

It muſt be admitted, that foreign birds, when brought to Europe, are often heard to a great diſadvantage; as many of them, from their great tameneſs, have certainly been brought up by hand, the conſequence of which I have already ſtated from ſeveral experiments. The ſoft-billed birds alſo cannot be well brought over, as the *ſuccedaneum* for inſects (their common food) is freſh meat, and particularly the hearts of animals.

I have happened, however, to hear the American mocking-bird in great perfection at Meſſ. Vogle's and Scott's, in Love-lane, Eaſtcheap.

This bird is believed to be ſtill living, and hath been in England theſe ſix years. During the ſpace of a minute, he imitated the woodlark, chaffinch, blackbird, thruſh, and ſparrow. I was told alſo, that he would bark like a dog; ſo that the bird ſeems to have no choice in his imitations, though his pipe comes neareſt to our nightingale of any bird I have yet met with.

With regard to the original notes, however, of this bird, we are ſtill at a loſs; as this can only be

* I cannot but think, that there would be a demand for theſe birds in China, as the inhabitants are very ſedentary, and bird cages are commonly repreſented as hanging in their rooms. have been informed, by a Tyroleze, that his beſt market for Canary birds was Conſtantinople.

known by thoſe who are accurately acquainted with the ſong of the other American birds.

Kalm indeed informs us, that the natural ſong is excellent *; but this traveller ſeems not to have been long enough in America to have diſtinguiſhed what were the genuine notes: with us, mimics do not oſten ſucceed but in imitations.

I have little doubt, however, but that this bird would be fully equal to the ſong of the Nightingale in its whole compaſs; but then, from the attention which the *mocker* pays to any other ſort of diſagreeable noiſes, theſe capital notes would be always debaſed by a bad mixture.

We have one mocking bird in England, which is the ſkylark; as, contrary to a general obſervation I have before made, this bird will catch the note of any other which hangs near it; even after the ſkylark note is *fixed*. For this reaſon, the bird-fanciers often place the ſkylark next one which hath not been long caught, in order, as they term it, to keep the caged ſkylark *honeſt*.

The queſtion, indeed, may be aſked, why the wild ſkylark, with theſe powers of imitation, ever adheres to the parental note; but it muſt be recollected, that a bird when at liberty is for ever ſhifting its place, and conſequently does not hear the ſame notes eternally repeated, as when it hangs in a cage near another. In a wild ſtate therefore the ſkylark adheres to the parental notes; as the parent cock attends the young ones, and is heard by them for ſo conſiderable a time.

* Vol. I. p. 219.

I am

I am aware alſo, that it may be aſked, how birds originally came by the notes which are peculiar to each ſpecies. My anſwer, however, to this is, that the origin of the notes of birds, together with its gradual progreſs, is as difficult to be traced, as that of the different languages in nations.

The loſs of the parent-cock at the critical time for inſtruction hath undoubtedly produced thoſe varieties, which I have before obſerved are in the ſong of each ſpecies; becauſe then the neſtling hath either attended to the ſong of ſome other birds; or perhaps invented ſome new notes of its own, which are afterwards perpetuated from generation to generation, till ſimilar accidents produce other alterations. The organs of ſome birds alſo are probably ſo defective, that they cannot imitate properly the parental note, as ſome men can never articulate as they ſhould do. Such defects in the parent bird muſt again occaſion varieties, becauſe theſe defects will be continued to their deſcendants, who (as I before have proved) will only attend to the parental ſong. Some of theſe deſcendants alſo may have imperfect organs; which will again multiply varieties in the ſong.

The truth is, as I before obſerved, that ſcarcely any two birds of the ſame ſpecies have exactly the ſame notes, if they are accurately attended to, though there is a general reſemblance.

Thus moſt people ſee no difference between one ſheep and another, when a large flock is before them. The ſhepherd, however, knows each of them, and can ſwear to them, if they are loſt; as can the Lincolnſhire goſherd to each gooſe.

As I now draw towards a concluſion of both my experiments and obſervations on the ſinging of birds; it may be poſſibly aſked, what uſe reſults either from the trouble or expence which they have coſt me; both of which I admit to have been conſiderable.

I will readily own, that no very important advantages can be derived from them; and yet I ſhall not decline ſuggeſting what little profit they may poſſibly be of, though at beſt they ſhould rather be conſidered as what Lord Bacon terms, *experiments of light, than of fruit.*

In the firſt place, there is no better method of inveſtigating the human faculties, than by a compariſon with thoſe of animals; provided we make it without a moſt ungrateful wiſh of lowering ourſelves, in that diſtinguiſhed ſituation in which we are placed.

Thus we are referred to the ant for an example of induſtry and foreſight, becauſe it provides a magazine of food for the winter, when this animal is in a ſtate of torpidity during that ſeaſon; nor are we leſs willing to ſuppoſe the ſong of birds to be ſuperior to our own muſical powers.

The notes of many birds are certainly very pleaſing, but can by no means ſtand in competition either with the human voice or our worſt muſical inſtruments; not only from want of the ſtriking effects of harmony in many excellent compoſitions; but becauſe, even when compared to our ſimple melody, expreſſion is wanting*, without which muſic is ſo languid and inanimate.

* The nightingale, indeed, is perhaps an exception to this general obſervation.

But

But to return to the uſes (ſuch as they are) which may ariſe from attending to the ſong of birds, or from the experiments which I have given an account of.

The firſt of theſe is too much neglected by the naturaliſt; for, if the bird is not caught, the only means often by which either the ſex or the ſpecies can be determined is the ſong. For example, if Monſ. Adanſon had informed us whether the European ſwallows, which he conceived were to be ſeen during the winter at Senegal, had the ſame notes with thoſe of Europe, it would have been going one ſtep further in proof of the facts which he and others ſo much rely upon.

Theſe experiments, however, may be ſaid to be uſeful to all thoſe who happen to be pleaſed with ſinging birds; becauſe it is clear, that, by educating a bird under ſeveral ſorts, we may often make ſuch a mixture, as to improve the notes which they would have learned in a wild ſtate.

It reſults alſo from the experiment of the linnet being educated under the Vengolina, that we may introduce the notes of Aſia, Africa, and America, into our own woods; becauſe, if that linnet had been ſet at liberty *, the neſtlings of the next ſeaſon would have adhered to the Vengolina ſong, who would again tranſmit it to their deſcendants.

* I know well, that it is commonly ſuppoſed, if you ſet a caged bird at liberty, it will neither be able to feed itſelf, nor otherwiſe live long, on account of its being perſecuted by the wild ones. There is no foundation, however, for this notion; and I take it to ariſe from an excuſe for continuing to keep theſe birds in confinement.

But

But we may not only improve the notes of birds by a happy mixture, or introduce thoſe which were never before heard in Great-Britain; as we may alſo improve the inſtrument with which the paſſages are executed.

If, for example, any bird-fancier is particularly fond of what is called the ſong of the Canary bird, which however muſt be admitted to be inferior in tone to the linnet, it would anſwer well to any ſuch perſon, if a neſtling linnet was brought up under a Canary bird, becauſe the notes would be the ſame, but the inſtrument which executes them would be improved.

We learn alſo, from theſe experiments, that nothing is to be expected from a neſtling brought up by hand, if he does not receive the proper inſtruction from the parent cock: much trouble and ſome coſt is therefore thrown away by many perſons in endeavouring to rear neſtling nightingales, which, when they are brought up and fed at a very conſiderable expence, have no ſong which is worth attending to.

If a woodlark, or ſkylark, was educated, however, under a nightingale, it follows that this charge (which amounts to a ſhilling per week *) might be in a great meaſure ſaved, as well as the trouble of chopping freſh meat every day.

A nightingale, again, when kept in a cage, does not live often more than a year or two; nor does he ſing more than three or four months; whereas the

* Olina ſpeaks of a paſte which is uſed in Italy for nightingales; but I cannot find that it ever anſwers with us; perhaps, they bring their nightingales up by hand, and ſo accuſtom them from their earlieſt infancy to ſuch food.

ſcholar pitched upon may not only be more vivacious, but will continue in ſong nine months out of the twelve.

I fear, however, that I have already dwelt too long upon theſe very minute and trifling advantages which may reſult from my experiments and obſervations; I ſhall therefore no longer defer ſubſcribing myſelf,

Dear Sir,

Your moſt faithful,

Humble Servant,

Daines Barrington.

Received January 8, 1773.

XXXII. *An Account of the* Tokay *and other Wines of* Hungary, *by* Sylveſter Douglaſs, *Eſquire: Communicated by* Edward Poore, *Eſq, F. R. S.*

Read June 10, 1773.

AS the growth, quantity, and value of the Tokay wine are little known in this country, and the popular notions concerning them are in general erroneous, it may be a matter both of ſome curioſity and uſe, to throw together what I was able to collect on this ſubject, in the country where it grows, as well as from the chief proprietors of it, both in Hungary and at Vienna. I ſhall ſubjoin a brief account of the other moſt remarkable Hungarian wines.

Geography. The town, or rather village, of Tokay, from whence this celebrated wine derives its name, ſtands at the foot, and to the eaſt of a high hill, cloſe by the conflux of the river Bodrug, with the Theis or Tibiſcus. In the Norimberg map of Hungary, it is erroneouſly placed between theſe rivers, for it is on the weſt ſide of both. The inhabitants are chiefly either Hungarians of the Proteſtant religion, or Greeks, who came originally from Turkey, but have

have been long ſettled here for the purpoſe of carrying on the wine trade. During the two laſt centuries, this country being almoſt the conſtant theatre of war, there was a citadel near the town, but now there is not the ſmalleſt veſtige of it remaining.

The hills on which the wine grows lie all to the weſt of the river Bodrog, and beginning cloſe by the town of Tokay, extend weſtward and northward from thence, and occupy a ſpace of perhaps ten Engliſh miles ſquare; but they are interrupted and interſperſed with a great many extenſive plains, and ſeveral villages, ſuch as Talia, Mada, Tarczal, Szombor, Benye, and Toleſwa. Near ſome of theſe, particularly Talia and Tarczal, the wine is better than what grows on the hill of Tokay, but it all goes under the ſame general name.

The vineyards extend beyond the 48th degree of northern latitude. Latitude.

The ſoil, on all the hills where the wine grows, is a yellow clayiſh earth, extremely deep, and there are interſperſed through it large looſe ſtones, which, as I was told, are limeſtone; but I had not an opportunity of examining them. Soil.

As the hills do not run in a regular chain, but are ſcattered among the intervening plains, you meet with all kinds of expoſures upon them, and there is wine on them all, except perhaps where they are turned directly towards the ſouth. Yet the general rule is, that the expoſures moſt inclining to the ſouth, the ſteepeſt declivities, and the higheſt part of thoſe declivities, produce the beſt wine. Theſe circumſtances ſhew the advantage of chooſing your wine on the ground. The late Mr. Wortley Mon- Expoſure.

tague was ſo well convinced of this, that a few years before his death he undertook a journey from England to Tokay, and continued there ſeveral months, in order to be ſure of having the beſt and moſt genuine wine.

Quantity. It is a vulgar error, that the Tokay wine is in ſo ſmall quantity, as never to be found genuine, unleſs when given in preſents by the court of Vienna. The extent of ground on which it grows is a ſufficient proof to the contrary. It is a common deſert wine in all the great families at Vienna, and in Hungary, and is very generally drank in Poland and Ruſſia, being uſed at table in thoſe countries, like Madeira in this.

Proprietors. Another vulgar error is, that all the Tokay wine is the property of the Empreſs Queen. She is not even the moſt conſiderable proprietor, nor of the beſt wine; ſo that every year ſhe ſells off her own, and purchaſes from the other proprietors, to ſupply her own table, and the preſents ſhe makes of it. The greateſt proprietor is the Prince Trautzon, an old man, at whoſe death, indeed, his eſtate will eſcheat to the crown; but many others of the German and Hungarian nobility have large vineyards at Tokay; moſt of the gentlemen in the neighbourhood have part of their eſtates there; the Jeſuits college at Ungwar has a conſiderable ſhare of the beſt wine; and beſides theſe, there are many of the peaſants who have vineyards, which they hold of the Queen, or other lords, by paying a tythe of the annual produce.

Grapes. There is never any red wine made at Tokay, and, as far as I recollect, the grapes are all white.

They

They are ſuppoſed to have a particular flavour, which I own I could not perceive, though they were beginning to be ripe when I was there, in the end of Auguſt (1768), and I have often eat of ſuch as have been brought to Vienna.

The vintage is always as late as poſſible. It commonly begins at the feaſt of St. Simon and Jude, October 28, ſometimes as late as St. Martin's, November 11. This is determined by the ſeaſon, for they have the grapes on the vines as long as the weather permits, as the froſts, which from the end of Auguſt are very keen during the nights, are thought to be of great ſervice to the wine. By this means it happens, that when the vintage begins, a great many of the grapes are ſhrivelled, and have, in ſome meaſure, the appearance of dried raiſins. Vintage.

There are four ſorts of wine made from the ſame grapes, which they diſtinguiſh at Tokay by the names of Eſſence, Auſpruch, Maſslaſch, and the common wine. Different ſorts.

The proceſs for making them is as follows.

The half-dried and ſhriveled grapes, being carefully picked out from the others, are put into a perforated veſſel, where they remain as long as any juice runs off by the mere preſſure of their own weight. This is put into ſmall caſks, and is called the Eſſence. 1. The Eſſence.

On the grapes from which the eſſence has run off, is poured the expreſſed juice of the others from which they had been picked, and then they tread them with their feet. The liquor obtained in this manner ſtands to ferment during a day or two, after which it is poured into ſmall caſks, which are kept 2. Auſpruch.

in

in the air for about a month, and afterwards put into the cellars. This is the Auſpruch.

3. Maſslaſch. The ſame proceſs is again repeated, by the addition of more of the common juice to the grapes which have already undergone the two former preſſures, only they are now alſo wrung with the hands, and this gives the Maſslaſch *.

The fourth kind. The fourth kind is made by taking all the grapes together at firſt, and ſubmitting them to the greateſt preſſure. It is chiefly prepared by the peaſants, who have not a ſufficient quantity of grapes, and cannot afford the time and apparatus neceſſary for making the different ſorts. It is, I believe, entirely conſumed in the country, and forms the common *Vin du pais*.

The Eſſence is thick, and never perfectly clear, very ſweet and luſcious. It is chiefly uſed to mix with the other kinds, and when joined to the Maſſlaſch, forms a wine equally good with the Auſpruch, and often ſold for it.

The Auſpruch is the wine commonly exported, and what is known in foreign countries under the name of Tokay. The following are the beſt rules for judging of it; though in this, and all ſimilar caſes, it requires experience to be able to put ſuch rules in practice.

1. The colour ſhould neither be reddiſh (which it often is) nor very pale, but a light ſilver.

2. In trying it, you ſhould not ſwallow it immediately, but only wet your palate and the tip of the

* I cannot anſwer for the orthography of this and the foregoing term, having written them by the ear, as they were pronounced.

tongue.

tongue. If it diſcover any acrimony to the tongue (or bite it), it is not good. The taſte ought to be ſoft and mild.

3. It ſhould, when poured out, form globules in the glaſs, and have an oily appearance.

4. When genuine, the ſtrongeſt is always of the beſt quality.

5. When ſwallowed, it ſhould have an earthy aſtringent taſte in the mouth, which they call the *Taſte of the root*. The Poles, particularly, are fond of this aſtringency and auſterity in their Tokay. There is ſo great a difference between the Tokay uſed in Poland and what I drank both at Tokay and Vienna, which, I am ſure, was of the beſt and moſt genuine kind, that I am inclined to think their wine is compoſed of the Maſslaſch, which, by the ſevere preſſure it ſuffers, muſt carry with it much of the aſtringent quality which, in all grapes, reſides in the ſkin, and a ſmaller proportion than uſual of the eſſence. But this is mere conjecture.

Beſides the qualities already mentioned, all Tokay wine has an aromatic taſte; ſo peculiar, that nobody, who has ever drank it genuine, can confound it with any other ſpecies of wine. The only ſpecies that bears a reſemblance to it grows, in a very ſmall quantity, in the Venetian Friule, and is only to be met with in private families at Venice, where, in the dialect of the place, it is called *Vin piccolit*.

The Tokay wine, both the Eſſence and Auſpruch, keeps to any age, and improves by time. I have drank of the latter at Vienna, which had been in the ſame cellar ſince the year 1686. It is never good till it is about three years old. Age.

Meaſure. All the ſorts are generally kept in ſmall caſks, called antheils, which *legally* hold 80 Hungarian mediæ, a meaſure containing about two thirds of an Engliſh quart. When you buy it of the gentlemen who are proprietors, you have commonly more than the legal quantity in the antheil; if from the Greek merchants, always leſs.

Price. The particular year, or vintage, and the age, vary the price of this as of all other wines.

The medium price of the antheil of Eſſence is between 60 and 70 ducats. It is ſometimes ſold on the ſpot for more than 100. Prince Radzivil paid 300 ducats for two antheils about four years ago. When the price is 60 ducats, and the antheil large meaſure, that is, about 90 mediæ, it is exactly a ducat the Engliſh quart.

The price of the Auſpruch is from 26 to about 30 ducats the antheil. This is at the rate of two florins, or near a crown the Engliſh quart. The Empreſs Queen, a little before I was at Tokay, purchaſed ſome of vintage 1754 at 33 ducats, and this was looked upon as an extraordinary price. The variety in the prices of the Eſſence and Auſpruch, accounts for the oppoſite relations of people, who ſay ſometimes that it coſts half a guinea, ſometimes five ſhillings, on the ſpot.

There are people who come every year from Poland, about the time of the vintage, to chooſe their own wine on the ground, and ſee it carefully managed. But it is a falſe opinion of many, that they contract for the wine of ſeveral years forwards: no ſuch thing has ever been practiſed. For theſe laſt twenty years the court of Peterſbourg has had an

agent, who reſides conſtantly at Tokay, for the purpoſe of buying wine. The preſent agent is a major in the Ruſſian ſervice, and formerly was a major-general. He commonly purchaſes every year from 40 to 60 antheils of Auſpruch, but never of any other ſort.

One of the principal proprietors, who reſides at Caſchaw *, told me, that he would engage to deliver the beſt old Tokay (i. e. Auſpruch), in bottles, at London, for a ducat a bottle, containing two thirds of a quart, or conſiderably cheaper in caſk, and would put his own ſeal on it, and inſure it.

It is much the beſt way to tranſport it in caſks; for when it is on the ſeas, it ferments three times every ſeaſon, and refines itſelf by theſe repeated fermentations. When in bottles, there muſt be an empty ſpace left between the wine and the cork, otherwiſe it would burſt the bottle. They put a little oil on the ſurface, and tie a piece of bladder on the cork. The bottles are always laid on their ſides in ſand.

I am perſuaded an Engliſh merchant, or company of merchants, would find their account in eſtabliſhing a correſpondence with one of the principal proprietors in the country, or in ſending an agent to reſide at Tokay, who might watch the opportunity of the good vintages, chooſe the beſt expoſures, and bargain with the proprietors themſelves. They ſhould have cellars there to keep the wine to a proper age, and an agent at Warſaw, and another at Dantzic, to receive it. This is the road it muſt take. The carriage, as I was informed, from Tokay to the fron-

* The Baron Veſey, preſident of the chamber of Caſchaw.

tiers of Poland, coſts the Poliſh merchants two ducats the antheil, and three florins of duty. The duties in Poland amount to about 14 florins the antheil. The carriage down the Viſtula cannot be very expenſive. Every charge included, I am perſuaded the beſt old Tokay might be ſold in London at half a guinea the quart bottle, which is 100 *per cent.* leſs than what the worſt often coſts.

Of the other Hungarian Wines.

There is not, I believe, in Europe any country which produces a greater variety of wines than Hungary. I have been told, at Vienna, of an entertainment given to the Empreſs Queen at Preſburg, by an Hungarian nobleman, where there were above a hundred different ſorts of wine produced, all the growth of that country. Many of them are of an excellent quality, and would undoubtedly be a ſource of great commerce and riches to that kingdom, if it enjoyed a maritime, inſtead of its inland, ſituation.

The moſt valuable white wines after the Tokay, are,

1. *The St. George wine*, which grows near a village of that name, about two German miles north of Preſburg, and in the ſame latitude with Vienna. This wine approaches the neareſt of any Hungarian wine to Tokay. Formerly they uſed to make Auſpruch at St. George; but this was prohibited by the court about ſixteen years ago, it being ſuppoſed that it might hurt the traffic of the Tokay wine. The prohibition ſhews, that the method of preparing the

the Tokay is thought to have a ſhare in its peculiar qualities.

2. *The Edenburg wine*, reſembling the St. George, but inferior in quality and value. Edenburg is a town ſituate about nine German miles north-weſt of Preſburg.

3. *The Carlowitz wine*, ſomething like that of the *Côte rotie* on the banks of the Rhone. Carlowitz is the ſeat of the metropolitan of the Greek church in Hungary. It ſtands on the banks of the Danube, between 45 and 46 degrees of latitude.

The beſt red wines are,

1. *The Buda wine*, which grows in the neighbourhood of the antient capital of the kingdom. This wine is like, and perhaps equal to, Burgundy, and is often ſold for it in Germany. A German * author of the laſt century ſays, that a great quantity of this wine uſed to be ſent to England in the reign of James I. over land by Breſlaw and Hamburg, and that it was the favourite wine both at court and all over England.

2. *The Sexard wine*, a ſtrong deep-coloured wine, not unlike the ſtrong wine of Languedoc, which is ſaid to be ſold at Bordeaux for claret. The Sexard wine on the ſpot coſts about 5 creuzers, or 2½ *d.* a bottle. It belongs to the Abbot of Conſtance, and is chiefly conſumed in Germany. Sexard is on the Danube, between Buda and Eſſeh.

3. *The Erlaw wine*, which is reckoned at Vienna almoſt equal to that of Buda. Erlaw is in Upper

* Hevelius Sileſiograph. 2 vols. art. *Breſlaw.*

Hungary, ſouth-weſt of Tokay, between 47 and 48 degrees of latitude.

4. *The Gros Wardein wine*, a ſtrong bodied wine, and very cheap. It belongs chiefly to the Duke of Modena, whoſe anceſtor got a large eſtate in this country, in grant from the Emperor Leopold, as a reward for his ſervices in the Hungarian wars. Gros Wardein is an old fortreſs near the confines of Tranſylvania, between 46 and 47 degrees of latitude.

XXXIII. *On*

XXXIII. *On the Figure and Compoſition of the Red Particles of the Blood, commonly called the Red Globules.* *By Mr.* William Hewſon, *F. R. S. and Teacher of Anatomy.*

Read June 17, and 24, 1773.

THE red particles of the blood in the human ſubject have, ſince the time of Leeuwenhoeck, been ſo generally allowed to be ſpherical, that in almoſt all books of phyſiology they are denominated red globules. A few authors, however, have at different times doubted whether they were ſpheres, and amongſt the reſt Father de la Torré, whoſe curious obſervations, together with his glaſſes, were preſented to the Royal Society, Anno 1766. As I flatter myſelf that I have made ſome new obſervations on theſe particles, I ſhall do myſelf the honour of communicating them to the Society.

It is a curious and important fact, that theſe particles are found ſo generally through the animal kingdom; that is, they are found in the human ſpecies, in all quadrupeds, in all birds, in all amphibious animals, and in all fiſh, in which animals they are red, and colour the blood. The blood even of inſects contains particles ſimilar in ſhape to thoſe of the blood of more perfect animals, but differing

fering in colour. In water inſects, as lobſters and ſhrimps, theſe particles are white; in ſome land inſects, as the caterpillar and the graſs-hopper they appear of a faint green, when in the veſſels as I am perſuaded from experiments. I have ſeen them in an inſect no bigger than a pin's head, and ſuſpect they exiſt almoſt univerſally through the animal kingdom.

What is ſo generally extended through the creation muſt be of great importance in animal œconomy, and highly deſerving the attention of every enquirer into the works of nature. This ſubject becomes the more intereſting from ſo much of reaſoning in the theory of medicine being built in the properties of thoſe particles.

It is by the microſcope alone that we can diſcover theſe particles; and as ſome dexterity and practice is required in the uſe of that inſtrument, there have not been wanting men of character and ingenuity, who, having been unſucceſsful in their own experiments, have queſtioned the validity of thoſe made more fortunately by others. Some have gone ſo far as to aſſert, that no credit can be given to microſcopes, that they deceive us by repreſenting objects different from what they really are. Theſe aſſertions, though not entirely without foundation, when we ſpeak of one ſort of microſcopes, are very unjuſtly applied to them all. In compound microſcopes, when the object is viewed through two or more glaſſes, if theſe glaſſes be not well adapted to the focus of each other, the figure of the object may be diſtorted; but no ſuch circumſtance takes place, when we view an object through a ſingle lens. All who uſe ſpectacles agree, that the figures of objects appear the ſame through them, as they do

do to the naked eye. And as the ſingle microſcope has, like the ſpectacle, but one lens between the eye and the object, there is no reaſon to ſuppoſe the one can deceive us more than the other. The compound, having a larger field, is more pleaſant than the ſingle microſcope for many purpoſes; but the ſingle ſhould be always preferred by thoſe who wiſh to aſcertain the figures of minute bodies. It was this inſtrument ſupported on a ſcroll, as delineated by Mr. Baker (Microſcope made eaſy, plate II. chap. 3.) that has been uſed in theſe experiments: and almoſt all the obſervations were made with lenſes, as they are prepared by ſome of our more ſkilful workmen in London. One obſervation only was made by means of thoſe globules made of glaſs, which the ingenious Father de la Torré preſented to the Royal Society, and which they were ſo obliging as to lend to me. Of theſe globules but two were fit for uſe, when they came into my hands, viz. that which, according to Father de la Torré, magnifies the diameter of the object 640 times, and that which magnifies 1280 times. The lenſes of the greateſt magnifying power made in London are thoſe of $\frac{1}{50}$th of an inch focus, which, even allowing eight inches to be the focal diſtance of the naked eye, magnifies the diameter of the object only 400 times; a power much inferior to what may be obtained by globules, and particularly by that globule, which according to Father de la Torré magnifies the diameter 1280 times; and this globule I have uſed in ſome of theſe experiments. But our lenſes, though inferior in magnifying power to theſe globules, are much ſuperior in diſtinctneſs; the

globules

globules are full of clouds, made by the ſmoke of the lamp uſed in preparing them, and the object can be ſeen only through the tranſparent parts of the globules, which makes it difficult to get a ſatisfactory view of it; this, with the trouble of adapting the objects to the focus of the glaſs, made me prefer our own lenſes for all the experiments mentioned in theſe ſheets, except one; and it is but doing juſtice to the ingenious gentleman abovementioned, to acknowledge, that the greater power of his glaſſes was found in that experiment more than to compenſate for their want of diſtinctneſs.

Theſe particles of the blood, improperly called globules, are in reality flat bodies. Leeuwhenhoeck and others have allowed, that in fiſh and in the *amphibia* they are flat and elliptical; but in the human ſubject and in quadrupeds almoſt all microſcopical obſervers have agreed in their being ſpherical. When we conſider how many ingenious perſons have been employed in examining the blood with the beſt microſcopes, it will appear wonderful, that the figure of theſe particles ſhould have been miſtaken; but our wonder will be leſſened, when we conſider how many obvious things are overlooked, till our attention is very particularly directed to them; and beſides, the blood in the human ſubject and in quadrupeds is ſo full of theſe particles, that it is with great difficulty we can ſee them ſeparate, unleſs we find out a method of diluting the blood. It is to ſuch a diſcovery that I attribute my ſucceſs in this enquiry; for, having examined the blood as it flows from the veſſels of the human body, it appeared a confuſed maſs, notwithſtanding

I ſpread

I ſpread it thin on a glaſs, or a piece of talk. It then occurred to me to dilute it, but not with water, for this I knew diſſolved the particles; but with ſerum, in which they remain undiſſolved. By the ſerum I could dilute it to any degree, and therefore could view the particles diſtinct from each other; and in theſe experiments I found that theſe particles of the blood were as flat as a guinea. I likewiſe obſerved that they had a dark ſpot in the middle, which Father de la Torré took for a hole; but, upon a careful examination, I found it was not a perforation, and therefore that they were not annular. I next made experiments by mixing theſe particles with a variety of other fluids, and examined them in many different animals; and the reſult of theſe experiments was, that their ſize is different in different animals, as is ſeen in plate XII, where they are repreſented of the ſize they appeared to my eye, when viewed through a lens of $\frac{1}{45}$ of an inch focus, which, allowing eight inches to be the focal diſtance of the naked eye, magnifies the diameter 184 times.

It may not be improper to obſerve here, that the accurate Leeuwenhoeck not having diluted the human blood, or that of quadrupeds, ſo as to ſee theſe particles ſeparate from each other, was thence not qualified to deſcribe them from his own obſervation, as he has done thoſe of fiſh and of frogs; and, ſuſpecting a round figure was more fit for circulating in our veſſels, was thence led to ſuppoſe theſe particles ſpherical in the human ſubject. But I ſhall hereafter be able to ſhew from his own words, that it is not his obſervations, but

his ſpeculative opinions, or his theory, that differs from what I have diſcovered by theſe experiments.

In plate XII. it appears, that of all the animals which I have examined, the particles are largeſt in the fiſh called a ſkate; next to the ſkate they are largeſt in a frog and a viper, and other animals of this claſs; they are ſomewhat ſmaller in the common fiſh, as the ſalmon, cod, and eel. In birds they are ſmaller than in fiſh; in the human ſubject ſmaller than in birds; and in ſome quadrupeds ſtill ſmaller than in the human ſubject. Leeuwenhoeck, ſpeaking of their ſize, ſays, he is confident the red particles of the blood are no larger in a whale than in the ſmalleſt fiſh *. And others have, ſince his time, ſaid they are of the ſame ſize in all animals; but it is evident, from comparing their ſize, as delineated in the abovementioned plate, that it differs conſiderably, and that they are not largeſt in the largeſt animals; for we find that in an ox they are not ſo large as in a man; and ſo far are they from being larger in the whale than in the ſmall fiſh, it appears probable, from comparing their ſize, as delineated p'ate XII. N° II. from a porpus, which belongs to the ſame genus as the whale, that they are ſmaller in thoſe animals than in fiſh. Neither is their ſize inverſely, as the ſize of the animal; for they are as large in an ox as in a mouſe. The difference in their ſize therefore depends on ſome other circumſtance than a difference in the ſize of the animal.

* Conſ. Arcan. Nat. p. 220.

As

As to their ſhape, I have already mentioned that they are flat in all animals, even in the human ſubject; of which any one may be convinced by repeating the following experiments.

EXPERIMENT I. Take a ſmall quantity of the ſerum of human blood, and ſhake a piece of the craſſamentum in it, till it is coloured a little with the red particles; then with a ſoft hair pencil ſpread a little of it on a piece of thin glaſs, and place this glaſs in the microſcope in ſuch a manner as not to be quite horizontal, but higher at one end than the other, by which means the ſerum will flow from the higher extremity to the lower, and as it flows, ſome of the particles will be found to ſwim on their flat ſides, and will appear to have a dark ſpot in their middle; others will turn over from one ſide to the other as they roll down the glaſs. No perſon who ſees them turn over can doubt of their being flat; he will ſee them in turning have all the phaſes that a flat body has; firſt he will ſee them on one ſide, then riſe gradually upon their edge, and turn over to the other ſide. I have in this way ſhewed their figure to a number of curious perſons, and particularly to many ſtudents of anatomy, who have attended lectures in London within the laſt ſix years.

If, inſtead of ſerum, the particles ſhould be diluted with water containing rather more ſalt than ſerum does; or if, inſtead of human blood, that of an animal with larger particles be uſed: then they will ſometimes be ſeen not only flat, but a little bended, like a crooked piece of money.

Theſe experiments not only prove that the particles of the blood are flat, and not globular; but likewiſe, by proving that they are flat, they ſhew that they are not fluid, as they are commonly believed to be; but, on the contrary, are ſolid; becauſe every fluid ſwimming in another, which is in larger quantity, if it be not ſoluble in that other fluid, becomes globular; this is the caſe where a ſmall quantity of oil is mixed with a larger quantity of water, or if a ſmall quantity of water be mixed with a large one of oil, then the water appears globular. And as theſe particles are not globular, but flat, they muſt be ſolid; a circumſtance that will appear ſtill more evident from future experiments.

It is neceſſary to remark, that in a few minutes after the particles are ſpread out on a glaſs, they run in cluſters, and ſtick to each other, and then they appear confuſed.

When one of theſe particles is attentively examined, while ſeparate from the reſt, it appears, as it lies on its flat ſide, to have a dark ſpot in the middle, and round that dark ſpot it is more tranſparent. This dark ſpot was believed to be a perforation, or the particle was ſuppoſed to be a hollow ring, by the ingenious Father de la Torré. But I find, from a great number of experiments, that the dark ſpot is a ſolid particle contained in a flat veſicle, whoſe middle only it fills, and whoſe edges are hollow, and either empty, or filled with a ſubtile fluid. This will be evident to every one who will carefully make the following experiments.

EXPERIMENT II. Take a drop of the blood of an animal that has large particles, as a frog, a fiſh, or, what is ſtill better, of a toad; put this blood on a thin piece of glaſs, as uſed in the former experiment, and add to it ſome water, firſt one drop, then a ſecond, and a third, and ſo on, gradually increaſing the quantity; and in proportion as water is added, the figure of the particles will be changed from a flat to a ſpherical ſhape. When much water is added, the veſicle will by degrees become thinner, and more tranſparent, and will at laſt be diſſolved. When the veſicle has thus aſſumed a ſpherical ſhape, it will roll down the glaſs ſtage ſmoothly, without thoſe phaſes which it had when turning over whilſt it was flat; and as it now rolls in its ſpherical ſhape, the ſolid middle particles can be diſtinctly ſeen to fall from ſide to ſide in the hollow veſicle like a pea in a bladder. Sometimes, indeed, inſtead of falling from ſide to ſide, the ſolid middle particle is ſeen to ſtick to one part of the veſicle; and in proportion as the veſicle, inſtead of being flat, aſſumes a ſpherical ſhape, its longeſt diameter is ſhortened as might be expected, on the ſuppoſition of its being hollow and flat.

After this experiment has been made on the blood of ſuch animals as have large veſicles, it may be made on human blood, where the water will be found to have the ſame effect; the veſicles will become ſpherical, the diameters of theſe ſpheres will be leſs than the largeſt diameter of the veſicle was, in its flat ſtate.

It is remarkable that more water is in general required to produce this change on the veſicles of the human blood than on thoſe of frogs, or other amphibious animals; and thoſe of the amphibia require ſtill more than thoſe of fiſh; for the ſubſtance of theſe veſicles being thicker and more coloured in man and in quadrupeds than in the amphibia, is therefore later in being diſſolved in water; and being thinneſt in fiſh, it thence moſt readily diſſolves. Thoſe who are deſirous of repeating theſe experiments had beſt begin with the blood of toads and frogs, whoſe veſicles are large, and remain ſome time without diſſolving in the water (when that is uſed with the above-mentioned precautions); by which means any one accuſtomed to microſcopical experiments may readily be ſatisfied of theſe curious circumſtances.

From the greater thickneſs of the veſicles in the human ſubject, and from their being leſs tranſparent when made ſpherical by the addition of water, and likewiſe from their being ſo much ſmaller than thoſe of fiſh or frogs, it is more difficult to get a ſight of the middle particles rolling from ſide to ſide in the veſicle, which has become round; but with a ſtrong light *, and a deep magnifier, I have diſtinctly ſeen it in the human ſubject, as well as in the frog, toad, or ſkate.

Since water makes theſe particles round, and makes the dark ſpot in their middle diſappear, it is evident the red particles of the human blood are not perforated, but that dark ſpot is owing to ſomething elſe than a hole: and this is likewiſe con-

* Theſe experiments were all made with day-light, in clear weather.

firmed by obſerving that although the particle does in an obſcure glaſs appear only to have a dark ſpot which might be ſuppoſed to be a hole, yet, with a very tranſparent lens, and a good light, after diluting the blood with ſerum, that middle part can be diſtinctly ſeen to be only of a deeper red than the reſt of the veſicle, and thence appears darker.

In theſe experiments, made by adding water to the blood, the middle particles appear to be leſs eaſily ſoluble in water than the flat veſicle which contains them; ſo that, a little time after the proper quantity of water has been added, the flat veſicles diſappear, leaving their middle particles; which ſeem to be globular, and very ſmall.

That theſe red veſicles of the blood, although flat, are not perforated, is evident likewiſe from a curious appearance which I have repeatedly obſerved in blood that has been kept three days in the ſummer ſeaſon, ſo that it was beginning to putrefy; the veſicles of this blood being diluted with ſerum, and examined with a lens $\frac{1}{50}$ of an inch focus, but more particularly when examined with M. de la Torré's glaſs, which, by his computation, magnifies the diameter 1280 times, were found to have become ſpherical; the diameter of theſe ſpheres was leſs than their largeſt diameter when flat, and their external ſurface was corrugated in ſuch a manner as to make them appear like ſmall mulberries.

I have ſeen the ſame appearance on mixing ſerum that had been kept three days in a warm place, and ſmelt putrid, with freſh-drawn human blood; the veſicle aſſumed this globular and mulberry-like appearance.

In

In theſe experiments on human blood beginning to putrefy, I have likewiſe obſerved ſome of theſe veſicles break into pieces without becoming ſpherical; and I have diſtinctly perceived the black ſpot in the center fiſſured through its middle, another proof that it is not a perforation.

In the blood of an eel, which was beginning to putrefy, I have ſeen the veſicles ſplit and open, and the particle in its center come out of the fiſſure.

As the putrefaction advances, thoſe veſicles which had become rough ſpheres, or like mulberries, and thoſe which had been merely fiſſured, each break down into ſmaller pieces. M. de la Torré ſeems to think they have joints, and break regularly into ſeven parts; and Leeuwenhoeck ſuſpected theſe globules, as he called them, were conſtantly made of ſix leſſer globules. But from obſervations I am convinced there is nothing regular or conſtant in the number of pieces into which they break. I have ſeen them fall into ſix, ſeven, eight, or more pieces, by putrefaction; for putrefaction breaks them down in the manner it deſtroys other animal ſolids.

I need hardly take notice, that the ſmall pieces into which the veſicles break are equally red as the veſicle itſelf. The theory of the red globules being compoſed of ſix ſerous ones compacted together, and the ſerous globules of ſix of lymph, has not the leaſt foundation, and is intirely overthrown by the ſimple experiment of mixing the blood with ſix or with thirty-ſix times its quantity of water; for the water diſſolving the globules ought to reduce them to yellow ſerum, or colourleſs lymph *: but it does

* See Gaubii Pathologia.

not; on the contrary, it is coloured red by theſe particles, even when uſed in much greater proportion than thirty-ſix parts of water to one of blood.

Theſe red veſicles of the blood have not only been commonly ſuppoſed globular and fluid, but they have with equal injuſtice been imagined to be oily and more inflammable than the reſt of the blood. That they are not oily is evident from their ſo readily diſſolving in water; and that they are not more inflammable than the reſt of the blood is manifeſt by burning them after they are ſeparated them from the reſt of the blood, which ſeparation may be effected by ſhaking the craſſamentum in the ſerum ſo as to diffuſe the particles through it; and then, by pouring off the clear ſerum, when they have ſubſided in it. I have ſeparated them in this manner, and compared their inflammability with that of inſpiſſated ſerum, and of dried coagulable lymph, and have not obſerved them more inflammable than the ſerum or the lymph; nor do they melt like oils, as ſome have ſuſpected, but burn like a piece of horn.

Some authors, who have written on the figure of theſe veſicles in quadrupeds, and in the human ſubject, have expatiated on the great advantages of their (ſuppoſed) ſpherical ſhape, in order for their more eaſy circulation; as it is probable that no form is preferable to a ſpherical one for eaſy motion. But as theſe veſicles are evidently not ſpherical but flat in all animals, we muſt believe that nature has ſome good purpoſe to anſwer by making them of that form.

It has been objected, that, notwithſtanding they appear flat out of the body, they may poſſibly be

globular in the body, while circulating; and it has been ſaid, that it is almoſt inconceivable that ſo many ingenious men ſhould at different times have viewed them through a microſcope, and have concluded them ſpherical, if they be really flat. But, however that may have happened, it is a fact that they are as flat in the body as out of it. Of this I am convinced by having repeatedly obſerved them whilſt circulating in the ſmall veſſels between the toes of a frog, both in the ſolar microſcope, and the more ſimple one abovementioned. I have ſeen them with their ſides parallel like a number of coins laid one againſt another. I have likewiſe in that animal, where they are elliptical, ſeen them move with one end foremoſt, and ſometimes with an edge turned towards the eye. I have moreover ſeen them, when entering a ſmall veſſel, ſtrike upon the angle between it and the larger trunk, and turn over with the ſame variety of phaſes that they have when turning over upon a piece of glaſs.

Upon this occaſion I may remark, that it has been ſaid by ſome microſcopical obſervers, that in paſſing through very ſmall veſſels they ſeem to alter their ſhapes, and to be lengthened. This concluſion, I ſuſpect, has taken its riſe from the obſerver having ſeen them with their edge turned towards his eye; in which caſe they would appear long and ſmall, as if lengthened by compreſſion, eſpecially to one who ſets out with the notion of their being globular. I have ſeen them in blood veſſels, which would admit only ſingle veſicles, move with difficulty, as if ſtreightened for room; but never ſaw them altered in their ſhape by the action of the veſſels.

If then they really be not globular but flat, and if water ſo readily alter their ſhape, whence is it that the ſerum has the property of preſerving them in that form which ſeems ſo neceſſary, becauſe it is ſo general through the animal creation?

It is principally by the ſalts of the ſerum that this effect is produced, as is proved by adding a ſmall quantity of any neutral ſalt to water, when the water is no longer capable of diſſolving thoſe particles, nor does it alter their ſhape when the ſalt is uſed in a certain proportion.

EXPERIMENT III. If a ſaturated ſolution of any of the common neutral ſalts be mixed with freſh blood, and the globules (as they have been called, but which for the future I ſhall call flat veſicles) be then examined in a microſcope, the ſalt will be found to have contracted or ſhriveled the veſicles, ſo that they appear quite ſolid, the veſicular ſubſtance being cloſely applied all round the central piece. In proportion as the ſolution of ſalt is diluted with water, it has leſs effect; and, when diluted with ſix, eight, ten, or twelve times its quantity of water, it produces no change in the figure of the veſicles, whoſe flat ſhape can then be ſeen even more diſtinctly than when mixed with ſerum itſelf.

The neutral ſalts, which, when diluted with water, have been obſerved to have the effects above-deſcribed, are Glauber's ſalt, Epſom ſalt, a ſalt formed of the volatile alkali and the vitriolic acid, common nitre, cubic nitre, a ſalt made with the volatile alkali and the nitrous acid, as well as the ſalts made with the nitrous acid and magneſia, or

with the nitrous acid and chalk, and alſo common ſalt, digeſtive ſalt of Sylvius, and a ſalt made with vinegar and the foſſil alkali. Theſe experiments were ſufficient to convince me, that this property was very general among thoſe ſalts which conſiſt of acid and alkali; and therefore it ſeemed unneceſſary to proſecute this enquiry farther *.

But acids and alkalies have different effects on theſe veſicles from what neutral ſalts have.

The fixed vegetable alkali, and the volatile alkali, were tried in a pretty ſtrong ſolution, and found to corrugate the veſicles; and in proportion as they were diluted, their effects became ſimilar to water alone, but it is not eaſy to find the point of ſtrength where the veſicles would remain unaltered in the ſolution. And here we may obſerve, that ſince theſe veſicles are found to diſſolve ſo readily in water, and not to be diſſolved in theſe ſolutions of alkali, it is a ſtrong argument againſt their being either oily or ſaponaceous, as they have been ſuſpected.

The effects of acids are very different. I have tried the vitriolic, nitrous, muriatic diſtilled vinegar, and the acid of phoſphorus; theſe, when much diluted, have the ſame effects as water in making the veſicles ſpherical; and, in proportion as they are leſs diluted, they diſſolve the veſicles without making them ſpherical, as water does. I never

* Theſe experiments were made by putting one drop of the ſaturated ſolution of the ſalt into a tea cup, and then adding diſtilled water by a few drops at a time; and to this mixture the ſerum of the blood, highly tinged with the red veſicles, was added.

could

could find any point of dilution where the acids like the neutral ſalts produced no change on the figure of the veſicles. This experiment is the more to be attended to, as theſe veſicles have been ſuppoſed to be oily and ſaponaceous, which is improbable, ſince they diſſolve more readily in acids than in alkalies.

Salts made with earth of alum, and any of the acids, always corrugate thoſe veſicles, unleſs they be very much diluted; when their effects are ſimilar to thoſe of the water alone, that is, they make the veſicles aſſume a ſpherical ſhape. I could not diſcover any point of ſtrength in theſe ſolutions where the particles would remain in them without being changed in their ſhape.

The ſame was obſerved of ſpirit of wine: ſome of the metalline ſalts, as copperas, ſublimate, and Roman vitriol, were tried; and when much diluted, their effects were not different from thoſe of water; but in proportion as the ſolution was ſtronger, they corrugated the veſicles more and more.

Urine, when containing much of its ſalts, has effects ſimilar to the ſerum; but in proportion as it is weaker, its effects are more like thoſe of water.

The uſe therefore of thoſe ſalts which enter into the compoſition of the blood is probably to preſerve the red veſicles in their flat form; for we muſt ſuppoſe ſome advantages attend that ſhape, ſince nature has made uſe of it ſo generally in the blood of different animals. And as both a very ſtrong ſolution of neutral ſalts and a very diluted one alters the ſhape of the veſicles, it is probable nature has

limited

limited the proportions of the water and the ſalts in our blood. A degree of latitude in theſe proportions however ſeems to be admitted; for I obſerved the veſicles equally unchanged when mixed with a ſolution of ſalts conſiſting of eight drops of water to one of the ſaturated ſolution, and when added to a mixture of fifteen drops of water to one of the ſame ſolution.

Not only the neutral ſalts in the blood are capable of preventing the ſerum from diſſolving the veſicles; but the mucilage or lymph with which the ſerum is ſo much impregnated, ſeems to contribute to the ſame effect.

When the veſicles have been made ſpherical, by being mixed with water, if a ſmall quantity of pretty ſtrong ſolution of a neutral ſalt be added, they are immediately ſhriveled, a few of them recover their former flat ſhape, but the greateſt part are contracted irregularly into ſmaller ſpheres. When theſe veſicles thus recover their ſhape, after having been a ſhort time mixed with water, they are generally more tranſparent, and appear thinner, a part of their ſubſtance having been diſſolved in the water; and thence it is more eaſy to diſtinguiſh the little ſolid particle which is contained in them. By this experiment I have had the pleaſure of convincing many curious perſons of the compoſition of this part of the blood, who were not quite ſatisfied from ſome of the other experiments.

I have mentioned above, and have ſhewn in plate XII. that theſe veſicles are of different ſizes in different animals. I have likewiſe obſerved, that they are not all of the ſame ſize in the ſame animal, ſome

ſome being a little larger than others; and ſome diſſolve in water more readily than others. In the ſame ſpecies of animals they even differ in ſize in the different periods of life. In a chicken on the ſixth day of incubation I found them larger than in a full-grown hen, as is repreſented in the plate; and I found them larger in the blood of a very young viper than in that of its mother, out of whoſe belly it was taken. I have not however been convinced from experiments, that there is any difference in ſize between thoſe of a child at its birth and thoſe of an adult perſon.

In the blood of ſome inſects the veſicles are not red, but white, as may eaſily be obſerved in a lobſter (which Linnæus calls an inſect); one of whoſe legs being cut off, a quantity of a clear *ſanies* flows from it; this, after being ſome time expoſed to the air, jellies, but leſs firmly than the blood of more perfect animals. When it is jellied, it is found to have ſeveral white filaments; theſe are principally the veſicles concreted, as I am perſuaded from the following experiment.

EXPERIMENT IV. If one of the legs of a lobſter be cut off, and a little of the blood be catched upon a flat glaſs, and inſtantly applied to the microſcope, it is ſeen to contain flat veſicles that are circular, like thoſe of the common fiſh, and have each of them a leſſer particle in their center as thoſe of other animals. But there is a curious change produced in their ſhape by being expoſed to the air; for ſoon after they are received on the glaſs, they are corrugated; or from a flat ſhape are changed into regular ſpheres, as is repreſented in plate XII. N° XII.

This

This change takes place ſo rapidly, that it requires great expedition to apply them to the microſcope ſoon enough to obſerve it.

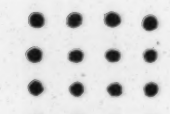
I have obſerved the *ſanies* or blood of a ſhrimp, by cutting off its tail, and found veſicles in it ſimilar to thoſe of the lobſter, which have been a ſhort time expoſed to the air. But I never could apply the blood ſo as to ſee them in their flat form; but, ſince they changed by expoſition to the air, I conjecture that like them they were flat in the blood veſſels; but being more ſuſceptible of changes from the contact of air, they were corrugated before I could get them applied to the microſcope.

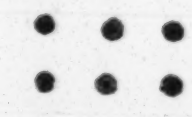
The ingenious Leeuwenhoeck has obſerved that in the blood of a graſhopper, its veſicles or globules, as he calls them, are green. I have ſeen the ſame circumſtance in the white caterpillar, whoſe ſerum appeared green when in its veſſels; but when let out from this animal or from a graſhopper, the colour cannot be diſtinguiſhed.

The ſmalleſt animal in which I have diſcerned theſe veſicles is an inſect no bigger than a pin's head, that is ſeen almoſt conſtantly in the river water which we have in London. This inſect, which is a ſpecies of the *Monoculus*, being put into a concave glaſs with a little water, and the rays of the ſun being made to paſs through it, the heart may be ſeen to beat, and the tranſparent blood or *ſanies* found to have a few veſicles, which appear to move one after the other; being made viſible, though tranſparent, by the light paſſing in ſuch a manner as to be refracted by them.

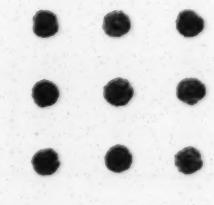
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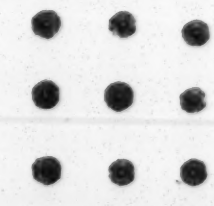
A comparative view of the **FLAT VESICLES** *of the B*
appear thro' a Lens $\frac{1}{23}$ *of an Inch Focus.*

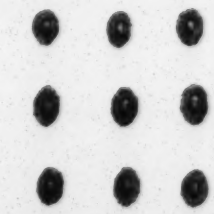
Fig. I.  *Their Size in an Ox, a Cat, an Ass, a Mouse, & a Bat.*

II.  ——— *in Man, in a Rabbit, a Dog, & a Porpus.*

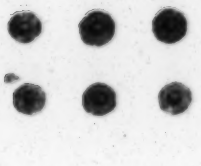
III.  ——— *in Birds, - viz a Pigeon, a Hen, a Chaffinch, & a Duck.*

IV.  ——— *in a Chick from the Egg, on the 6th. day of Incubation.*

V.  ——— *in the common Fish, as the Salmon, Carp, Eel.*

VI.  ——— *in a full grown Viper, & in a Turtle.*

Blood in different Animals, exhibiting their size & Shape as they

VII.  Their Size in a small Viper taken from the Belly of its Mother.

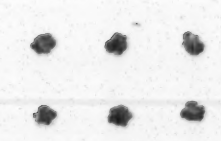
VIII.  ——— in a Slow Worm.

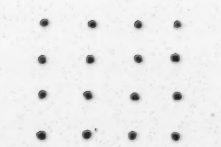
IX.  ——— in a Frog.

k.

X.  ——— in a Skate.

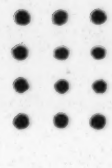
XI.  ——— in a Lobster

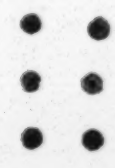
XII.  The Vesicles of the same Lobster, as they appear after being a short time exposed to the Air.

XIII.  The size of the Globules of Milk.

W. Brown Sculp.

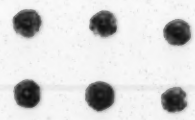
A comparative view of the **FLAT VESICL**
appear thro' a Lens $\frac{1}{23}$ *of an Inch Focus.*

Fig. I.  *Their Size in an Ox, a Cat, an Ass, a Mouse, &*

II.  ——— *in Man, in a Rabbit, a Dog, & a Po*

III.  ——— *in Birds, - viz a Pigeon, a Hen, a Chap*

IV.  ——— *in a Chick from the Egg, on the 6.th day of* '

V.  ——— *in the common Fish, as the Salmon, Ca*

VI.  ——— *in a full grown Viper, & in a Turtle.*

Since ſo ſmall an animal as this has theſe curious veſicles equally as the larger and more perfect animals, is it not probable that they are diffuſed through the whole animal creation? And what is found ſo generally amongſt animals muſt be of great uſe in their œconomy.

P. S.

Mr. Hewſon begs leave to add, that ſince theſe experiments open a new field for enquiry; and as he has already ſo far proſecuted the ſubject as to be perſuaded that he has thereby diſcovered not only the uſe of the lymphatic glands and of the Thymus, but alſo of the ſpleen: in order to have the opinions of the ingenious concerning the facts mentioned in this paper, he ventures to ſolicit, that ſuch gentlemen as are curious in natural knowledge, and particularly the Members of the Royal Society, would honour him with their company, at his houſe in Craven-ſtreet, in the Strand, where he will repeat the experiments before them.

XXXIV. *Account of the Effects of a Thunder-Storm, on the* 15*th of* March 1773, *upon the Houſe of Lord* Tylney *at* Naples. *In a Letter from the Honourable Sir* William Hamilton, *Knight of the Bath, his Majeſty's Envoy Extraordinary at the Court of* Naples, *and F. R. S. to* Mathew Maty, *M. D. Sec. R. S.*

Naples, March 20, 1773.

SIR,

Read June 17, 1773. ON Monday laſt, about half paſt ten at night, I had the ſatisfaction of being one, of many witneſſes, to ſeveral curious phænomena, occaſioned by the lightning having fallen on Lord Tylney's houſe, in this city. It was on his Lordſhip's aſſembly night; ſo that moſt of the nobility of this country, many of the foreign miniſters, foreigners of diſtinction, particularly Engliſh, were preſent at the time of the exploſion; to be ſure there were not leſs than two hundred and fifty in the apartments, and, including ſervants, the whole number under Lord Tylney's roof could not be leſs than five hundred. The lightning paſſed through nine rooms, ſeven of which were crouded with

with parties at cards, or converſing; it was viſible in every one, notwithſtanding the quantity of candles, and has left in all, evident marks of its paſſage. Many of the company were ſenſible of a ſmart ſtroke, like that of electricity, and ſome complained for ſeveral days after, of a pain they felt from that ſtroke, but no one received any eſſential hurt; a ſervant, indeed, of the French ambaſſador's houſe has a black mark on his ſhoulder and thigh, from a ſtroke he received on the ſtair-caſe; and another ſervant, who was aſleep on the ſame ſtair-caſe, his head reclining againſt the wall, had the hair entirely ſinged from it on that ſide.

The confuſion at the moment was, as you, Sir, may well imagine, very great: the report, which ſeems to have been equally heard in every room, was certainly as loud as that of a piſtol; and every one flying the room they were in, thinking the danger there, met of courſe in the door-ways, and ſtopped all paſſage. A Poliſh prince, who was playing at cards, hearing the report (as he thought of a piſtol), and feeling himſelf ſtruck, jumped up, and, clapping his hand to his ſword, put himſelf in a poſture of defence. I was ſitting on a card-table, and converſing with Monſieur de Sauſſure, Profeſſor of Natural Hiſtory at Geneva; we happened to be looking different ways, and each of us thought that the bright light and report was immediately oppoſite to us: and, upon enquiry, I found that every one was perſuaded that the greateſt exploſion had been directly before him. I thought that an Indian cracker had been fired, and Monſieur de Sauſſure thought it the report of a piſtol; but hearing, amidſt other

confuſed cries and noiſes, a voice ſaying, *Un fulmine, un fulmine!* we began to examine the gallery in which we were, and ſoon diſcovered that the gilding of the corniſh had been affected, for in the corners, and at every junction, it was quite blackened; thoſe that had been ſitting under the corniſhes were covered with the ſhining particles of the varniſh that went over the gilding, and which was thrown off in ſmall duſt, at the moment of the exploſion. There was a ſmell of ſulphur in Lord Tylney's apartments, but not very conſiderable; I thought there was more in the apartment above, which Monſieur de Sauſſure and I viſited immediately after, and where we found the ſame operation had been performed on the gildings. It is very certain that the profuſion of gildings, which is remarkable in this houſe, and the bell-wires, prevented the lightning from making more uſe of the company to conduct it in its courſe. I will endeavour to give you as clear an account as I am able, of what I ſaw the next morning, with Monſieur de Sauſſure, when we examined together, moſt carefully, the whole of Lord Tylney's houſe; which you are at liberty to communicate to our reſpectable Society, if you think it worthy of its attention. There never was, I believe, an accident of the kind, that proved more clearly, the exact ſimilitude of lightning and the electrical fluid, in all their operations.

The beſt apartments of this country have uſually a broad corniſh of lacker, or falſe gold, round their coved ceilings. Wood, covered with white plaiſter, a ſilvered leaf, and a yellow varniſh, compoſes this magnificent corniſh; a band of the ſame ſort, but much

much narrower, goes round the hangings, and down the corners, where it is double, which you will immediately comprehend, by caſting your eye on the ſection of the gallery, marked A, of the incloſed drawing [ſee TAB. XIII]. The chairs, ſophas, frames of pictures, tables, &c. are uſually of the ſame ſort of gilding, at leaſt they were ſo here. By ſending you the dimenſions of each room of Lord Tylney's apartment, you will ſee on what a prodigious ſurface of gilding the lightning ſpread itſelf in its courſe; for you muſt add the ſame quantity of gilding in the apartments over Lord Tylney's, which are of the ſame dimenſions, as richly decorated, and as much damaged; though we remarked that Lord Tylney's rooms that had ſuffered moſt, did not always correſpond with thoſe that ſuffered moſt in the upper apartments. Monſieur de Sauſſure and I began our examination on the flat roof of the houſe, compoſed of a kind of ſtucco, on which there was no ſign of damage; neither was there on any of the chimneys. A tin gutter, with many ſpouts of the ſame metal, projecting about three feet, is immediately under this roof, and each of the ſpouts is ſupported by a ſmall iron rod, or cramp, inſerted in the wall underneath, and above by two wires of about the ſize of a goole-quill, and which likewiſe go into the wall. We obſerved a wire of this ſort melted; and it ſeems highly probable, that the lightning found its way into the houſe at this place. We obſerved alſo, from the roof, that, though the houſe ſtands high, it is neverthelefs commanded by many cupolas, and higher buildings; which, with other circumſtances, makes it highly probable, that only a

portion

portion of the lightning, the great exploſion of which was heard all over Naples, had been conducted through our aſſembly. In the garrets under the aſtrico, or flat roof, we could perceive no ſigns of damage; under them, in the rich apartment immediately over Lord Tylney's, and conſiſting of the ſame number of rooms, the gilding of the corniſhes, bands, chairs, ſophas, &c. exhibited exactly the ſame appearance as in Lord Tylney's, which ſhall be particularly deſcribed preſently. The account of the appearances at the moment of the exploſion, given us by the few people that were in the apartments at the time, correſponded perfectly with what we had ſeen below.

Lord Tylney's apartment conſiſts of five rooms on a line, and four others, going off at a right angle from the fourth room of that line. The lightning ſeems to have entered the firſt room of the five towards the north, and which is under that part of the gutter where we ſuſpected it to have entered that part of the houſe. The five rooms of this line are of the ſame breadth, 23½ feet, and the four others are 14½ feet broad. The firſt room is 18½ feet long. The gilt corniſh of the whole apartment is in general 9½ inches broad, and the gilt band that goes round the hangings 2¾ inches in breadth. The corniſh of the room is only blackened at the joints, particularly the corners, and where there was any flaw or crack in the gilding. The ſmall bands, which appear by their colours to have conducted the lightning down from the corniſh in eight different parts of this room are (as in the other rooms) 14 feet

feet high, and theſe are what I ſhall call hereafter vertical deſcents, as marked in the drawing of the ſection of the room A. The gilding of a ſopha in this room is likewiſe blackened. We found, that whenever a chair or ſopha had been affected, it was owing to its having been, at the time of the explo-ſion, in contact with the gilt band, and that the point of contact was continually marked by a black ſpot, both on the chair and band.

The ſecond room, or gallery, in which Monſieur de Sauſſure and I were at the time the lightning fell, is $33\frac{1}{2}$ feet long; the gilding of the corniſh is much damaged, particularly on that ſide in contact with the bell wire. The lightning in this room had ten vertical deſcents, and paſſed over the gilding of two chairs, two ſopha's, and the frames of two marble tables, the white marble of which, at thoſe parts which were in contact with the gilding, is tinged yellow, and ſuch parts of the damaſk of the chairs and ſopha's as were in contact with the gild-ing, and had nails underneath, are ſinged. I found, upon enquiry, that moſt of thoſe who were ſitting upon theſe particular chairs and ſopha's were ſen-ſible of a ſmart ſtroke.

The third room, a ſection of which is marked A. in the drawing, is thirteen feet long, the corniſh much damaged, a ſopha and two chairs damaged; nine vertical deſcents in this room.

The fourth room is $14\frac{1}{2}$ feet long; the corniſh is damaged, and the lightning had nine vertical de-ſcents in this room likewiſe, the bands being much damaged.

The fifth room is twelve feet two inches long; the corniſh much hurt, particularly on the ſide in contact with the bell wire; there are no vertical bands in this room, therefore no apparent vertical deſcents of the lightning.

The ſixth room, which is the angle to the fourth, is much hurt in the corniſh; it is fourteen feet long, and has no gilt bands.

The ſeventh room nineteen feet ½ long; no ſigns of damage on the gilding of the corniſh, or in any other parts of the room, except the bell wire, which was melted, and ſeemed alone to have conducted the whole accumulated force of the lightning to the corniſh of the next room.

Eighth room; a ſection of which is repreſented in the drawing under B. The corniſh being overcharged, and the lightning, finding no compleat vertical conductor, jumped from the picture frame over the door to the gilding of the door caſe, which gilding is ſix inches wide; and on one ſide where the gilding ended, it knocked out a piece of wood, which is likewiſe burnt, or rather ſinged. The track of the lightning is evidently marked on the white wall, as if by the flame of a candle, black and yellow; and the ſame ſort of tinge is viſible (as repreſented in the drawing) on each ſide of the gilding of the door. We remarked that the picture over the door was the only one in the room that was in contact, or near the corniſh.

The ninth room is eighteen feet long; the corniſh is hurt, and the lightning deſcended from it to a picture frame over the door, and from thence to another, which was the greateſt jump that we remarked;

ed; its paſſage is clearly worked on the wall, as is repreſented in the drawing C, and the diſtance from one picture frame to the other is thirteen inches. It went then to the other picture frame, and down to the gilding of the door caſe, which is ſurrounded in part, having made a hole in the wall, which however it did not pierce.

The lightning ſeems to have been much ſtronger in theſe two rooms than in any other; and, as they were ſervants rooms, there were not above two or three people in them at the time of the exploſion.

Underneath theſe apartments we found no traces of the exploſion, except on the wall of the room directly under the door caſe of Lord Tylney's eighth room, where a piece of the plaiſtered wall of about ſix inches ſquare was beat out, and ſcattered about the floor.

Under this room again was a wall in a damp waſh-houſe, where moſt probably the lightning communicated with the earth, and diſperſed itſelf.

Thus have I followed it through its courſe, and will take my leave of you; but firſt I muſt tell you, that I have ſucceeded in diſcharging my battery of nine bottles over the cornices of two of my rooms, which repreſents in miniature exactly what we ſaw in ſuch perfection at Lord Tylney's.

An excellent electrical machine that I had of Mr. Nairne is the wonder of this country; as

they had never before ſeen electrical experiments in perfection.

I am,

SIR,

with great regard and eſteem,

your moſt obedient

humble ſervant,

Wm. Hamilton.

Explanation of PLATE XIII.

A. Section of the third room. B. Section of the eighth room. C. Section of the ninth room. D. Bands by which the lightning deſcended vertically. E. The gilding affected moſt in the corners and marbled with black, where the lightning was moſt powerful. F. Bell-wire melted. G. Jump of the lightning traced on the white wall. H. Vertical deſcents of the lightning marked on the white walls; the greateſt jump, from the firſt picture frame to the ſecond, is 13 inches. I. Hole made in the wall by the lightning. K. Piece of wood beat out of its place and burnt. L. Sopha, the gilding blackened.

Receiued


B
F
G

E

A

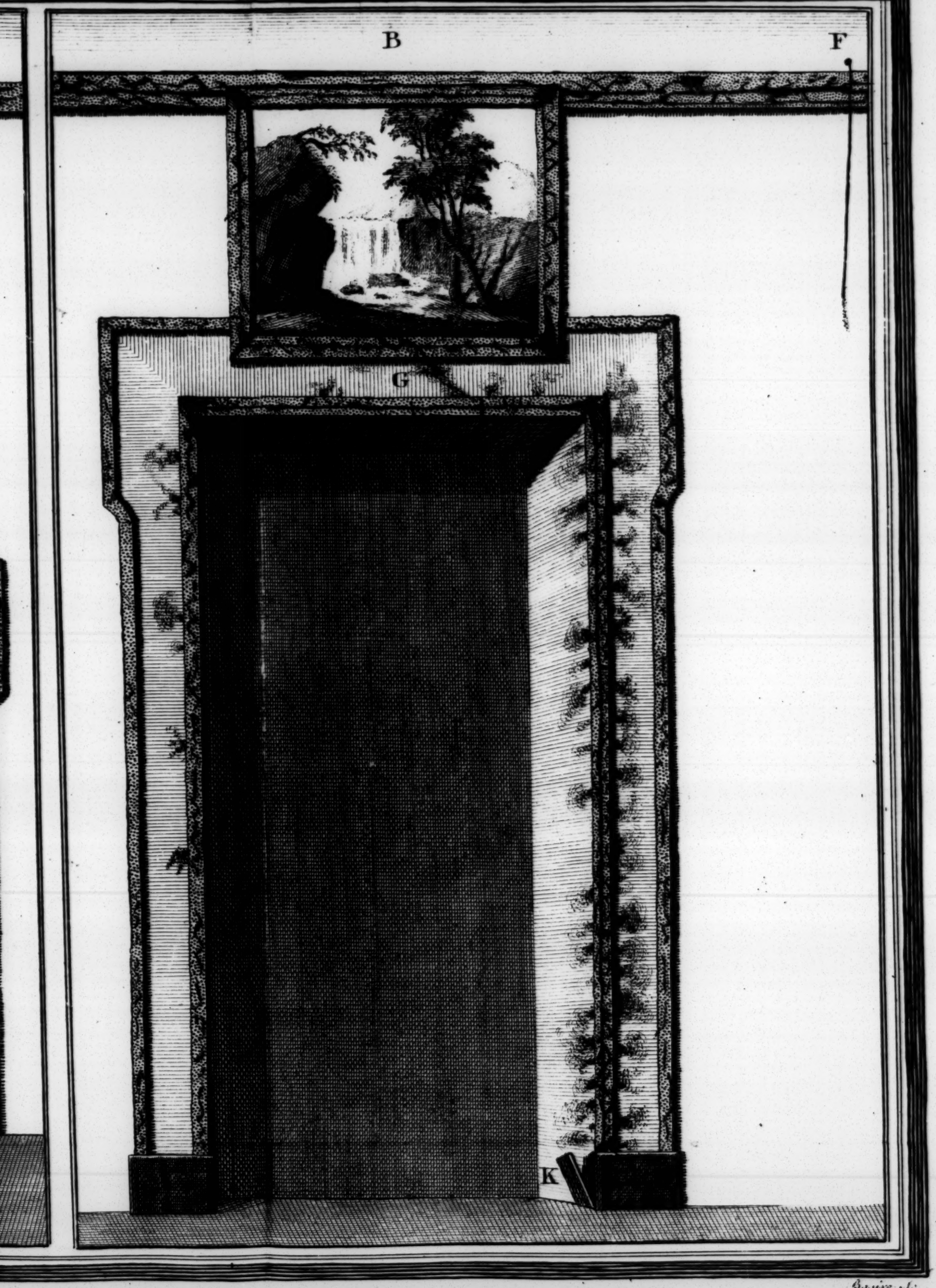
C

H

H

H

D = Vertical Descent



Basire sc.

Received, April 22, 1773.

XXXV. *An Extract of a Letter from Dr.* Nooth *to Dr.* Franklin, *F. R. S. on ſome Improvements in the Electrical Machine.*

Read June 24, 1773.

IT muſt undoubtedly appear extraordinary, that, in the preſent age, when the ſtudy of electricity is become ſo general, and the advances that have been made in the ſcience are ſo very conſiderable, I ſhould attempt to recall your attention to the ſtructure of the electrical machine. But I believe it muſt be allowed, that, notwithſtanding the remarkable progreſs that has of late been made in electrical purſuits, the machine ſtill remained the moſt imperfect part of the apparatus. The conſtruction of it has been in general left to the workman, who has ſeldom been in a capacity of making thoſe improvements in it, which it certainly admits of.

The ſubject, however, ſeems well worth the attention of the electricians themſelves; as a knowledge of the means of correcting the capricious ſtate of their machines will enable them to purſue

their electrical inquiries with more certainty, ſucceſs, and ſatisfaction.

A proſpect of diſcovering the cauſe of the common uncertainty in the action of electrical machines, induced me, ſome months ſince, to make ſome obſervations on the appearances produced by a machine in motion. Being well convinced, that the electric fire, which we receive from a machine, is derived from the cuſhion, and from ſuch parts as communicate with it, I firſt attended to the effects which the glaſs and rubber had on each other.

My inquiries, however, were not directed towards an inveſtigation of the cauſe of that accumulation of electric matter, in conſequence of the friction of the glaſs on the rubber, as I looked on that circumſtance as a ſecret in nature, no leſs impenetrable than gravitation itſelf; but I endeavoured to find out the beſt method of increaſing the excitation of a glaſs, and of taking from it that fire for electrical purpoſes which might be collected on its ſurface.

It is evident, that the electric matter is excited in the inſtant that the glaſs paſſes over the rubber, and that it becomes ſenſible to us by its adhering to the revolving ſurface of the glaſs. It likewiſe appeared to me highly probable, that the quantity of fire, which we find on the glaſs in motion, is not the whole of that which is excited by the paſſage of the glaſs on the rubber. The luminous appearance in the angles between the glaſs and rubber, and which is extremely diſtinct in a dark room, rendered it next to certain, that a part of the excited electric fluid returns immediately to the

cuſhion

cuſhion without performing a revolution with the glaſs; and that, of courſe, a circulation of the fire is thus kept up in the ſubſtance of the cuſhion in the common method of conſtructing the machines.

To be certainly convinced of this, I attempted to make the paſſage of the fire from the glaſs to the anterior part of the cuſhion, or to that part which correſponds with the aſcending ſide of the cylinder, demonſtrable, by placing a piece of ſilk between the glaſs and cuſhion. This ſilk was larger than the cuſhion; and part of it was allowed to adhere, by the attraction of the electric fire, to the aſcending part of the cylinder. My view in doing this was to cut off, in that part, the immediate communication between the excited glaſs and cuſhion, and by that means render the circulation of electric matter viſible, which I ſuſpected to take place in the machine; as it was thus forced to turn over the looſe edge of the ſilk before it could return to the cuſhion. The event anſwered my expectation; and I then perceived, that the greateſt part of the excited fluid was commonly re-abſorbed by the fore-part of the cuſhion without becoming ſenſible on the ſuperior part of the glaſs.

Having thus verified my ſuppoſition by actual experiments with ſilken flaps of different ſizes, I endeavoured to diſcover a method of preventing that circulation of the electric fluid, and, if poſſible, of obliging the whole, or the greater part of it, that is once excited, to make the revolution with the glaſs. This, indeed, the ſilk, when of conſiderable breadth, in ſome meaſure effected; but I thought that this obſtruction to the immediate re-

turn of the fire might be rendered more complete by increaſing the thickneſs of the ſilk, or by applying to it ſome nonconducting ſubſtance, that might confine the excited fluid more perfectly to the ſurface of the revolving cylinder.

Bees-wax being a nonconducting ſubſtance eaſily procured, I rubbed the ſilken flap with it, and found, as I expected, that the return of the fire to the cuſhion at the anterior part of the machine was by that means much diminiſhed, and conſequently the excitation of the glaſs was apparently increaſed. The addition, however, of more ſilk was ſtill more effectual in confining the fire to the glaſs; and when it was employed ten or twelve times doubled, it ſeemed to deny any paſſage from the glaſs to the cuſhion.

As I thus diſcovered the method of remedying the common defect in the conſtruction of the anterior part of the cuſhion, I next attended to that part which correſponds with the deſcending ſide of the cylinder. Being convinced that this part of the rubber was alone concerned in the excitation, I imagined that the reverſe of what was neceſſary anteriorly ſhould be adopted in the ſtructure of the poſterior part; that, inſtead of placing nonconducting ſubſtances between the glaſs and cuſhion, we ſhould here make the afflux of the electric matter as great as poſſible, by the application of the moſt perfectly conducting bodies. Confining therefore the amalgam to that place where the glaſs firſt comes in contact with the rubber, I placed ſome tinfoil cloſe to the amalgam, and, bending it back, ſecured it to the metallic plate below the cuſhion. By this means

means the electric matter found an eaſy acceſs to the place of excitation; and the effect of the machine was thereby incredibly increaſed. A piece of leather, covered with amalgam, and fixed to the poſterior part of the rubber, in ſuch a manner as to allow about an inch of it to paſs under the cylinder, anſwered every purpoſe of the tinfoil; and, as it was not liable to be corroded by the mercury, like tinfoil, it was on that account much preferable.

From the above experiments it was apparent that the excitation was altogether performed by the poſterior portion of the cuſhion; and that the anterior part, when made of conducting ſubſtances, re-abſorbs the greater quantity of the excited matter. In the ſtructure therefore of electrical machines, we ſhould always have a free electric communication behind, to facilitate the excitation; and the moſt perfectly nonconducting ſubſtances before, to prevent the re-abſorption. To anſwer theſe intentions, it will perhaps be adviſeable to make the cuſhion of ſilk, ſtuffed with hair, and to lay ſome metallic conductor round the poſterior part, that a free acceſs might be allowed to the electric matter coming to the place of excitation from the inferior part of the machine. Cuſhions, made in this manner, and then covered with ſilk ten or twelve times doubled, are much more powerfully excitant than any others that I have yet tried. Various other methods, however, may be purſued in the conſtruction of the rubber; but it ſhould be an invariable rule, to place nonconducting bodies before, and conducting ſubſtances behind, the cylinder. From the preceding principles, it follows, that the ſupport

to

to the rubber ſhould likewiſe have its conducting and nonconducting ſide. For this purpoſe, it may be neceſſary to employ baked wood, and to cover the poſterior half with tinfoil. The place of excitation will be thus ſufficiently ſupplied with electric matter; and the cylinder will not be robbed of a part of the excited fire, before that fire has made a revolution with the glaſs.

By attending to the place where the excitation is effected, it muſt appear evident, that the amalgam is only to be laid on the poſterior part of the cuſhion; its preſence, indeed, would be uſeleſs, if not injurious, in any other ſituation. It will, however, be found ſomewhat difficult to confine the pure amalgam to the poſterior part of the rubber; but if it is mixed with a little hair-powder and pomatum, it pretty perfectly keeps its place. The ſtrewing the amalgam thus prepared on the glaſs, as it revolves, is perhaps the beſt method of applying it; as, by that means, it is in a great meaſure prevented from paſſing on to the nonconducting ſubſtances that are placed before. Should any of the amalgam be carried forward by the revolution of the glaſs, it ſhould be carefully removed. The neceſſity of keeping that part free from conducting bodies cannot be too much inſiſted on; and, when freſh amalgam is applied as before mentioned to the proper part of the rubber, the flap ſhould be held down during half a dozen turns of the machine, leſt it might collect ſome of the amalgam before it is properly fixed. It is a probable conjecture, that, when the flap of ſilk is covered with amalgam, part of the amalgam, which is not immediately ſubſervient to the excitation,

tion, acts as a conductor in restoring the fire again to the cushion; and that thus, by an improper disposition of it, we suppress, instead of increasing, the quantity of the excited matter.

In short, when an electrician attends to the preceding principles in the construction of his rubber, and to the proper disposition of the amalgam, he has nothing to fear from the humidity of the atmosphere, as his machine will work equally well in all kinds of weather. The rest of the electrical apparatus may be made according to the directions that have been given by the different electrical writers. Each has had his favourite machine; and, perhaps, no one has been yet contrived that has not had its peculiar advantages.

Received May 13, 1773.

XXXVI. *Properties of the Conic Sections; deduced by a compendious Method. Being a Work of the late* William Jones, *Esq; F. R. S. which he formerly communicated to Mr.* John Robertson, *Libr. R. S. who now addresses it to the Reverend* Nevil Maskelyne, *F. R. S. Astronomer Royal.*

SIR,

Read June 24, 1773.

YOU well know that the curves formed by the sections of a cone, and therefore called CONIC SECTIONS, have, from the earliest ages of geometry, engaged the attention of mathematicians, on account of their extensive utility in the solution of many problems, which were incapable of being constructed by any possible combination of right lines and circles, the magnitudes used in plane geometry. The properties of these curves are become far more interesting within the two last centuries, since they have been found to be similar to those which are described by the motions of the cœlestial bodies in the Solar system.

Two

Two different methods have been taken by the writers who have treated of their properties; the one, and the more antient, is to deduce them from the properties of the cone itſelf; the other is to conſider the curves, as generated by the conſtant motion of two or more ſtrait lines moving in a given plane, by certain laws.

There are various methods of generating theſe curve lines *in plano*; one method will give ſome properties very eaſily; but others, with much trouble: while, by another mode of deſcription, ſome properties may be readily derived, which, by the former, were not ſo eaſily come at: ſo that it appears there may be a manner of deſcribing the curves ſimilar to the Conic Sections, by the motion of lines on a plane, which, in general, ſhall produce the moſt eſſential properties, with the greateſt facility.

That excellent mathematician, the late William Jones, Eſq; F. R. S. had drawn up ſome papers on the deſcription of theſe curves, or lines of the ſecond kind, very different from what he gave in his *Synopſis Palmariorum Matheſeos*, publiſhed in the year 1706; or from that of any other writer on this ſubject. A copy of theſe papers he was pleaſed to let me take about the year 1740. He had not finiſhed them as he intended; but, in their preſent ſtate, they appear of too much conſequence to be loſt; as, it is much to be feared, his own copy, together with many other valuable papers, are; and therefore, I am deſirous of preſerving them in the Philoſophical

Tranſactions, in the manner I at firſt tranſcribed them; although, I am aware, they might have been put into a form more pleaſing to the generality of readers: I have indeed annexed larger diagrams than what accompanied the author's copy, in order to render the lines more diſtinct, as all the relations are to be repreſented in a ſingle figure, of each kind.

Mr. Jones, having laid down a very ſimple method of deſcribing theſe curves, ſeems to have been deſirous of arriving at their properties in as expeditious a way as he could contrive; and therefore he has uſed the algebraic method, in general, of reducing his equations; and, on ſome occaſions, has uſed the method of fluxions, to deduce ſome properties chiefly relating to the tangents; and, by a judicious uſe of theſe, he has very much abridged the ſteps which otherwiſe he muſt have taken, to have deduced the very great variety of relations he has obtained: theſe he intended to have arranged in tables, from whence an equation expreſſing the relation between any three or more lines of the Conic Sections, might be taken out as readily as a logarithm out of their tables; this he has only partly executed; but it may eaſily be continued by thoſe who are deſirous to have it done, and are ſufficiently acquainted with what follows.

From the Houſe of the Royal Society,
April 29, 1773.

THE DESCRIPTION OF LINES OF THE SECOND KIND.

LET the right lines AD, AQ, be drawn on a plane, at any inclination the one with the other. See PLATE XIV. Fig. 1, 2, 3.

In AD, AQ, take A*a*, AM, of any given magnitude, and draw MN parallel to AD.

On the points A, *a*, let two rulers AP, *a*P, revolve, and cut MN, AQ, in N and Q, ſo that AQ be every-where equal to MN.

Then ſhall the interſection P of the rulers deſcribe lines of the ſecond kind, or curves of the firſt kind.

Where the right-line A*a*, is the firſt, or tranſverſe diameter.

The point C, biſecting the diameter A*a*, is the center.

The right-line PD, drawn parallel to AQ, is the ordinate to the diameter A*a*.

The part AD, or CD, of the diameter, is the abſciſs, when reckoned to begin from A to C, or from C to A.

The right line B*b* drawn from the center C parallel to the ordinate PD, and terminated in the curve, is called the ſecond, or conjugate diameter.

Thoſe diameters to which the ordinates are perpendicular, are called the axes.

And AM is the parameter to the diameter A*a*.

THE

THE PROPERTIES OF LINES OF THE SECOND KIND.

1. Put $Aa=d=2AC=2t$; $Bb=\delta=2BC=2c$; $AM=2p$; $PD=y$; $CD=x$; $AD=u$.
Then $\overline{PD}^2=\frac{p}{t}\times AD a$.

Or $yy=\frac{p}{t}\times u\times\overline{d\mp u}=2pu\mp\frac{p}{t}uu=\frac{p}{t}\times\overline{\pm tt\mp xx}=\pm pt\mp\frac{p}{t}x^2$.

For $PD=\frac{AM\times AD}{MN}=\frac{AQ\times Da}{Aa}$ (by sim. $\triangle$s). Th. $\overline{PD}^2=\frac{AM}{Aa}\times AD a$.

2. Consequently $\frac{t}{p}yy=\pm tt\mp xx=du\mp uu=\pm\frac{1}{4}dd\mp xx$.

3. Hence $\frac{t}{p}y\dot{y}=\mp x\dot{x}=\overline{t\mp u}\times\dot{u}=x\dot{u}$.

4. And $pt=cc$, or $2pd=\delta\delta$; for when $y=c=\frac{1}{2}\delta$, then $x=0$.

5. Therefore $\frac{\overline{PD}^2}{AD a}=\frac{yy}{\pm tt\mp xx}=\frac{p}{t}=\frac{2p}{d}=\frac{2pd}{dd}=\frac{pt}{tt}=\frac{\delta\delta}{dd}=\frac{cc}{tt}=\frac{\overline{BC}^2}{\overline{AC}^2}$.

6. The curve line whoſe property is $yy+\frac{p}{t}uu-2pu=0$,
(where the abſciſſa begins at the curve),
Or $yy+\frac{cc}{tt}xx-cc=0$, (where it begins at the center),
is called an Ellipſis. This curve returns into itſelf. For when $x=0$, then $y=c$; and when $y=0$, then $x=t$. Which can happen but two ways.

7. The curve line whoſe property is $yy-\frac{p}{t}uu-2pu=0$,

Or $yy-\frac{cc}{tt}xx+cc=0$, is called an Hyperbola. This curve ſpreads out infinitely. For y increaſes as x increaſes; and has four legs tending contrary ways: for xx, or yy, may be produced as well from $-x$, or $-y$; as from $+x$, or $+y$.

8. If the point a, is ſuppoſed to be at an infinite diſtance from A, ſo that a ruler aP moves in a parallel poſition to AD; then is $yy=2pu$, or $yy-2pu=0$, the property of the curve deſcribed, and is called a Parabola. This curve ſpreads out infinitely; for y increaſes as u increaſes.

9. Let

9. Let Aa, Pp, be any two firſt diameters; Bb, Qq, their ſecond diameters.
PLATE XIV. Fig. 4.
Draw the ordinates PD, QE, to the diameter Aa, and the ordinate Pd, to the diameter B b.
Let PT be a tangent, and PM be perpendicular, to the curve, in P; PT cutting Aa, Bb, produced, in T, t; and PM, in M, m.
Put the ſubtangent DT $= s$, $dt = \sigma$.
Let AT $= r$, PM $= \pi$, CE $= w$, CT $= q = x \pm s = t \pm r$,
Put s, s', = ſine and coſine of the angle MPD, or angle PmB,
R = tabular radius.

Then CDT $= xs = \overline{t \mp u} \times s = \frac{t}{p} yy = \frac{tt}{cc} yy = \pm tt \mp xx = u \times \overline{d \mp u} =$ ADa.

For $\dot{u} \times \overline{t \mp u} = \left(\frac{t}{p} y\dot{y} =\right) \alpha y\dot{y}$. Therefore $\left(\frac{\dot{u}}{\dot{y}} = \frac{\alpha y}{t \mp u} =\right) \frac{\alpha y}{x} = \frac{s}{y}$ (by ſim. $\triangle$s).

And C$dt = y\sigma = \frac{p}{t} xx = \frac{cc}{tt} xx = \pm cc \mp yy =$ Bdb. For $\left(\frac{\dot{u}}{\dot{y}} =\right) \frac{\alpha y}{x} = \frac{x}{\sigma}$.

10. Hence $\overline{AC}^2 = tt = x \times \overline{x \pm s} = x \times \overline{t \pm r} = xq =$ DCT.

11. And $\overline{BC}^2 = cc = y \times \overline{y + o} = y \times$ C$t = d$Ct.

12. Conſequently $\frac{\overline{Pd}^2}{Bdb} = \frac{xx}{\pm cc \mp yy} = \frac{tt}{cc} = \frac{\overline{AC}^2}{\overline{BC}^2}$.

13. Alſo $\overline{CE}^2 = ww = \left(\overline{DT}^2 \times \frac{\overline{QE}^2}{\overline{PD}^2} = \overline{DT}^2 \times \frac{AE a}{AD a} =\right) ss \times \frac{tt - ww}{sx} = sx = \frac{tt}{cc} yy =$ AD$a =$ CDT.

14. Therefore AE$a = \left(\frac{\overline{CE}^2}{\overline{DT}^2} \times$ AD$a = \frac{sx}{ss} \times sx =\right) xx = \overline{CD}^2$.

15. And $\overline{QE}^2 = \left(\frac{\overline{PD}^2}{AD a} \times$ AE$a =\right) \frac{cc}{tt} xx = \frac{p}{t} xx$.

In

In the general ſchemes. PLATE XV. and Fig. 5. PLATE XIV.

16. Let A*a*, B*b*, be the longeſt and ſhorteſt axes.
Draw Cϱ perpendicular to the tangent PT, cutting it in ϱ.
Put CP = T ; CQ = C ; Cϱ = ϱ.

Then $C\varrho = \varrho = \left(\frac{Ct \times CE}{CQ} = \frac{cc}{yy} \times \frac{ty}{cC} =\right) \frac{tc}{C} = \frac{AC \times CB}{CQ}$.

Hence the parallelogram, under the two axes, is equal to the parallelogram under any two diameters.

17. Draw the tangents AN, *an*, to any vertices A, *a*, meeting any diameters P*p*, Q*q*, produced in V, U, and *v*, *u*, and the tangent PT in N, *n*.

Then $AU = \left(\frac{CA \times EQ}{CE} =\right) \frac{px}{p}$. And $AV = \left(\frac{CA \times PD}{CD} =\right) \frac{ty}{x}$.

18. Alſo $CU = \left(\frac{AU \times CQ}{EQ} =\right) \frac{cc}{y}$. And $CV = \left(\frac{CP \times CA}{CD} =\right) \frac{tT}{x}$.

19. Hence $PV = (CV \backsim CP =) T \times \frac{t \backsim x}{x}$. And $pV = T \times \frac{t+x}{x}$.

Alſo $QU = (CU \backsim CQ =) C \times \frac{c \backsim y}{y}$. And $qU = C \times \frac{c+y}{y}$.

20. When A*a* and B*b* are the longeſt and ſhorteſt axes; and when $y = p$,
Then $xx = tt \mp \frac{t}{p} yy$ will become $tt \mp pt = tt \mp cc$, which call ff.
And CD = x, will become CF = Cf = f.
The points F, f, are called the Focii.

21. Hence $AF = af = \pm t \mp f$; $Af = aF = t + f$.

22. Alſo $\overline{CF}^2 = \overline{Cf}^2 = ff = \pm tt \mp cc = \pm tt \mp pt$.
And in the ellipſis, $\overline{AC}^2 = \left(\overline{BC}^2 + \overline{CF}^2 =\right) \overline{BF}^2$.
in the hyperbola, $\overline{CF}^2 = \left(\overline{AC}^2 + \overline{BC}^2 =\right) \overline{BA}^2$.
Hence, a circle deſcribed from B, with the diſtance AC in the ellipſis, or from C, with the diſtance AB in the hyperbola, will cut the axis A*a* in the focii F, f.

23. Draw

23. Draw FP, fP, from the focii F, f, to any point P of the curve; and draw the conjugate diameters Pp, Qq.

Put $PF = z$; $Pf = v$; $PC = T$; $QC = C$; $\frac{f}{t} = \varphi$; $\frac{f}{c} = \gamma$. Then

$$\overline{Pf}^2 = vv = yy + xx + 2xf + ff = tt + 2fx + \frac{ff}{tt}xx = TT + ff + 2fx.$$

$$\overline{PF}^2 = zz = yy + xx - 2xf + ff = tt - 2fx + \frac{ff}{tt}xx = TT + ff - 2fx.$$

For $yy = \left(\frac{cc}{tt} \times \overline{tt \backsim xx} =\right) \frac{tt \backsim ff}{tt} \times \overline{tt \backsim xx}$.

24. $Pf = v = \left(\sqrt{tt + 2fx + \frac{ff}{tt}xx} =\right) t + \frac{f}{t}x = t + \varphi x = \frac{tt + fx}{t}$.

$PF = z = \left(\sqrt{tt - 2fx + \frac{ff}{tt}xx} =\right) t - \frac{f}{t}x = t - \varphi x = \frac{tt - fx}{t}$.

25. $PF \pm Pf = z \pm v = 2t = Aa$.

26. $Pf^2 + \overline{PF}^2 = vv + zz = 2yy + 2xx + 2ff = 2tt + 2\varphi\varphi xx = 2TT + 2ff$.

27. $\overline{Pf}^2 - \overline{PF}^2 = vv - zz = 4fx = \overline{v + z} \times \overline{v - z} = 2t \times \overline{v \mp z}$.

28. $FPf = zv = z \times \overline{2t \mp z} = 2tz \mp zz = tt \backsim \varphi\varphi xx = \overline{tt \backsim xx} \times \frac{cc}{tt}xx$

$= cc \backsim \frac{ff}{cc}yy = \frac{tt}{cc}yy + \frac{cc}{tt}xx = \overline{CE}^2 + \overline{EQ}^2$

$= \overline{CQ}^2 = CC = T \times P$.

29. Let $m = t - z = v - t = \frac{fx}{t} = \varphi x = \pm \frac{1}{2}v \mp \frac{1}{2}z$

$= \frac{fx}{\sqrt{cc + ff}} = \frac{x}{t}\sqrt{tt \mp cc} = \sqrt{tt \mp zv}$

$= \sqrt{tt \mp CC} = \sqrt{ff + cc - zv}$.

30. Hence $\dot{z} = -\dot{v} = \varphi\dot{x} = -\frac{f}{t}\dot{x} = \frac{fy}{px}\dot{y}$.

And $\dot{v} = -\dot{z} = \varphi\dot{x} = \frac{f}{t}\dot{x} = -\frac{fy}{px}\dot{y}$.

31. $AC = t = \frac{1}{2}z \pm \frac{1}{2}v = \frac{cc}{p} = \sqrt{cc \pm ff} = \frac{fx}{m} = \frac{cx}{\sqrt{cc \mp yy}}$

$$= \frac{c}{y}\sqrt{sx} = \sqrt{x \times x \mp s} = \frac{y^2 + \sqrt{y^4 + 4p^2x^2}}{2p} = \frac{yy}{2p} + \sqrt{\frac{y^4}{4p^2} + xx}.$$

32. $\overline{CD}^2 = \overline{Pd}^2 = xx = \overline{\frac{ttyy}{ccs}}^2 = \overline{\frac{tyy}{ps}}^2 = \pm tt \mp \frac{t}{p}yy = \frac{tt}{cc} \times \overline{cc \mp yy}$

$$= \frac{ff+cc}{cc} \times \overline{cc \mp yy} = tt \mp \frac{tt}{cc}yy = \frac{mm}{\varphi\varphi} = \frac{tt}{ff}mm$$

$$= \frac{cc+ff}{ff}mm = \frac{tt}{ff} \times \overline{tt \mp zv} = \frac{tt}{ff} \times \overline{tt \mp CC} = \overline{\frac{tt}{q}}^2$$

$$= \frac{tt}{cc} \times zv - \frac{tt}{cc}yy = \frac{t}{p} \times zv - \frac{t}{p}yy = \frac{tt}{ff} \times \overline{t - z}^2$$

$$= \frac{tt}{ff} \times \overline{v - i}^2 = \overline{\frac{tt \mp tz}{f}}^2 = \overline{t \mp u}^2 = \overline{\sqrt{tt + \frac{1}{4}ss} - \frac{1}{2}s}^2$$

$$= \frac{tt}{ff} \times \overline{tt \mp \frac{tt}{cc}\pi\pi} = \frac{t^4}{c^4} \times \frac{SS}{RR}\pi\pi.$$

33. And $\dot{x} = -\frac{tyy}{px} = -\frac{ttyy}{ccx} = -\frac{tt\dot{y}}{qq} = -\frac{t\dot{z}}{f} = \frac{t\dot{v}}{f}$.

34. $\overline{PD}^2 = \overline{Cd}^2 = yy = \frac{p}{t}sx = \frac{cc}{tt}sx = \frac{cc}{tt} \times \overline{\pm tt \mp xx} = \pm cc \mp \frac{cc}{tt}xx$

$$= \pm pt \mp \frac{p}{t}xx = \frac{2cc}{t}u \mp \frac{cc}{tt}uu = \frac{\pm tt \mp ff}{tt} \times \overline{\pm tt \mp xx}$$

$$= \frac{cc}{tt} \times zv - \frac{cc}{tt}xx = \frac{cc}{tt} \times CC - \frac{cc}{tt}xx = \frac{cc}{ff} \times \overline{zv - cc}$$

$$= \frac{cc}{ff}nn = \frac{pt}{ff}nn \text{ (putting } nn = zv - cc)$$

$$= \frac{tt\pi\pi - c^4}{ff} = cc \mp \frac{tt}{cc} \times \frac{SS}{RR}\pi\pi = \frac{S'S'}{RR}\pi\pi.$$

35. And $\dot{y} = -\frac{px\dot{x}}{ty} = -\frac{ccx\dot{x}}{tty} = \frac{px\dot{z}}{fy} = -\frac{px\dot{v}}{fy}$.

36. Alſo $\frac{ff}{cc}yy = zv - cc = \frac{ff}{tt} \times \overline{tt \mp xx} = ff \mp \frac{ff}{tt}xx$

$$= nn = ff \mp mm = CC - cc.$$

37. $\overline{PC}^2$

37. $\overline{PC}^2 = TT = xx + yy = xn \pm cc \mp \frac{cc}{tt}xx = tt - ff + \frac{ff}{tt}xx = cc + \varphi\varphi xx$
$= cc + tt \backsim zv = cc + mm = cc + tt \backsim CC$
$= tp + \varphi\varphi xx = 2cc + ff \backsim zv = 2pt + ff \backsim zv.$

38. And $\dot{T} = \frac{\varphi\varphi x\dot{x}}{T} = \frac{\varphi\varphi x\dot{x}}{\sqrt{tp + \varphi\varphi xx}} = \left(\frac{\varphi\varphi x}{T} \times -\frac{ty\dot{y}}{px} =\right) \frac{ffy\dot{y}}{ptT} = -\frac{ffy\dot{y}}{ccT}.$

39 Alſo $\overline{CQ}^2 + \overline{CP}^2 = (cc + tt =) \overline{CB}^2 + \overline{CA}^2.$

40. $CT = q = \frac{tt}{x} = (tt \times \frac{f}{tm} =) \frac{ft}{m} = \frac{ft}{t \backsim z} = \frac{ft}{\sqrt{tt \backsim zv}}$

$Ct = \frac{cc}{y} = \frac{cf}{n} = \frac{cf}{\sqrt{zv - cc}}.$

41. And $\dot{q} = -\frac{q\dot{x}}{x} = -\frac{qq\dot{x}}{tt} = \frac{qqy\dot{y}}{ccx} = \frac{qqy\dot{y}}{ptx} = -\frac{tt\dot{x}}{xx}$ (for $tt = qn$).

42. $AT = (\pm CT \mp CA =) \pm \frac{tt}{x} \mp t = \frac{t}{x} \times \pm\overline{t \mp x} = \pm\frac{ft}{m} \mp t = \pm\frac{ft}{t \mp z} \mp t.$

$aT = (CT + Ca =) \frac{tt}{x} + t = \frac{t}{x} \times \overline{t + x} = \frac{ft}{m} + t = \frac{ft}{t \backsim z} + t.$

43. $FT = (\pm CT \mp CF = \pm \frac{ft}{m} \mp f = \frac{\pm ft \mp fm}{m}) = \frac{fz}{t \backsim z} = \frac{fz}{m} = \frac{tz}{x}.$

$fT = (CT + Cf = \frac{ft}{m} + f = \frac{ft + fm}{m} =) \frac{fv}{v \backsim t} = \frac{fv}{m} = \frac{tv}{x}.$

On Aa deſcribe a circle, draw the tangent TP', and draw CP'.

Continue DP to P', and at right angles to TP', draw FR',

Then $FR' = \left(\frac{FT}{CT} \times CP' = \frac{tz}{x} \times t \times \frac{x}{tt} =\right) z = FP.$

$fr' = \left(\frac{fT}{CT} \times CP' = \frac{tv}{x} \times t \times \frac{x}{tt} =\right) v = fP.$

44. $\mathrm{DT} = s = \frac{tyy}{px} = \frac{ttyy}{ccx} = \frac{\pm tt \mp xx}{x} = \frac{tnn}{fm}$.

$dt = \sigma = \frac{pxx}{ty} = \frac{ccxx}{tty} = \frac{cc \mp yy}{y} = \frac{cmm}{fn}$.

45. $\mathrm{AN} = \left(\frac{\mathrm{C}t}{\mathrm{CT}} \times \mathrm{AT} =\right) \frac{cm}{nx} \times \overline{t - x} = \frac{c}{n} \times \overline{f - m}$.

$an = \left(\frac{\mathrm{C}t}{\mathrm{CT}} \times a\mathrm{T} =\right) \frac{cm}{nx} \times \overline{t + x}$.

46. $\mathrm{VN} = \left(\frac{\mathrm{PV} \times \mathrm{C}t}{\mathrm{PC}} = \frac{t - x}{\mathrm{T}} \times \frac{\mathrm{T}}{x} \times \frac{fc}{n} =\right) \frac{fc}{nx} \times \overline{t - x} = \frac{fc}{nm} \times \overline{f - m}$

$= \frac{fc}{\sqrt{zv - cc} \times \sqrt{it - zv}} \times \overline{f - \sqrt{tt - zv}}$

$\mathrm{UN} = \mathrm{C}t = \frac{cc}{y}$.

47. $\mathrm{AD} = (\pm \mathrm{AC} \mp \mathrm{CD} = \pm t \mp \frac{tm}{f} =) \frac{t}{f} \times \pm \overline{f \mp m}$

$a\mathrm{D} = (\mathrm{AC} + \mathrm{CD} = t + \frac{tm}{f} =) \frac{t}{f} \times \overline{f + m}$.

48. Produce PF, pf, so meet the curve in Π, π;

Draw $\Pi\Delta$ perpendicular to Aa.

Put $\mathrm{F}\Pi = z'$, $f\pi = v'$, $\mathrm{F}\Delta = x'$.

Now, $\mathrm{FD} = (\pm \mathrm{CF} \mp \mathrm{CD} =) \pm f \mp x = \pm f \mp \frac{tm}{f} = \frac{tz - cc}{f}$

$f\mathrm{D} = f + \frac{tm}{f} = \frac{tv - cc}{f}$.

Then $\mathrm{F}\Delta = x' = (\mathrm{C}\Delta - \mathrm{CF} =) \frac{tm}{f} - f = \left(\frac{t}{f} \times \overline{t - z'} - f =\right) \frac{cc - tz'}{f}$

$f\Delta = f + \frac{tm}{f} = \frac{tv' - cc}{f}$.

49. From

49. From F, f, draw FR, fr, perpendicular to the tangent PT, and cutting it in R, r.

$$FR=\lambda=\frac{FT\times C\varrho}{CT}=\frac{cz}{c}=\frac{cz}{\sqrt{zv}}=\frac{cz}{\sqrt{2tz-zz}}=c\sqrt{\frac{z}{v}}$$
$$=c\sqrt{\frac{t-\varphi x}{t+\varphi x}}=\sqrt{\frac{ptz}{zt-z}}=\frac{z\varrho}{t}$$
$$fr=\lambda'=\frac{fT\times C\varrho}{CT}=\frac{cv}{c}=\frac{cv}{\sqrt{zv}}\text{ - - - - }=c\sqrt{\frac{z}{v}}$$
$$=c\sqrt{\frac{t+\varphi x}{t-\varphi x}}=\sqrt{\frac{ptv}{2t-v}}.$$
$$\dot\lambda=\frac{tc\dot z}{v\sqrt{zv}}.\quad\text{For }\lambda^2=\frac{c^2z}{v}.\quad\text{Th. }2\lambda\dot\lambda=\left(\frac{c^2v\dot z-c^2z\dot v}{vv}=\right)\frac{2c^2t\dot z}{vv}.$$

50. $$TR=\left(\frac{AT\times RF}{AN}=\frac{tnz}{mC}=\right)\frac{ttnz}{fcx}=\frac{ttn}{fx}\sqrt{\frac{z}{v}}=\frac{tn}{m}\sqrt{\frac{z}{v}}.$$
$$Tr=\frac{aT\times rf}{an}=\frac{tn}{m}\sqrt{\frac{v}{z}}.$$

51. Draw PM perpendicular to the tangent PT, meeting the axes Aa, Bb, in M, m.

$$DM=\left(\frac{PD\times PD}{TD}=\right)\frac{ccm}{ft}=\frac{cc}{ft}\sqrt{tt\pm zv}=\frac{cc}{tt}x=\frac{p}{t}x.$$
$$=\frac{S}{R}\pi=p\mp\frac{p}{t}u=\frac{cc}{tf}\sqrt{tt\mp\frac{tt}{cc}\pi\pi}.$$
$$dm=\left(\frac{PD\times Pd}{DM}=\right)\frac{ttn}{fc}=\frac{tt}{cc}\sqrt{zv-cc}=\frac{tt}{cc}y=\frac{t}{p}y.$$

52. $$CM=(CD\mp DM=x\mp\frac{cc}{tt}x=)\frac{tt\mp cc}{tt}x=\frac{ff}{tt}x=\frac{f}{t}m$$
$$=\frac{f}{t}\sqrt{tt\mp zv}=\frac{f}{t}\times\sqrt{t\mp z}=\frac{f}{t}\times\sqrt{tt\mp\frac{tt}{cc}\pi\pi}.$$
$$cm=(dm\mp cd=\frac{tt}{cc}y\mp y=)\frac{tt\mp cc}{cc}y=\frac{ff}{cc}y=\frac{f}{c}n=\frac{f}{c}\sqrt{zv-cc}$$

53. $$FM=(CF\text{ or }CM=f\text{ or }\frac{fm}{t}=)\frac{f}{t}\times\sqrt{t\mp m}=\frac{fz}{t}=\pm f\mp\frac{ff}{tt}x.$$
$$fM=(fC+CM=f+\frac{fm}{t}=)\frac{f}{t}\times\sqrt{t+m}=\frac{fv}{t}=f+\frac{ff}{tt}x$$

54. $\overline{Fm}$

54. $\overline{Fm}^2 = \overline{fm}^2 = (\overline{cf}^2 + \overline{cm}^2 =) \; ff + \frac{ffnn}{cc} = \frac{ffzv}{cc} = \frac{ff}{cc} \times cc.$

55. $AM = (AC \backsim CM =) \; t \backsim \frac{fm}{t} = \frac{tt \backsim fm}{t} = t \backsim \frac{ff}{tt} x.$

$aM = (aC + CM =) \; t + \frac{fm}{t} = \frac{tt + fm}{t} = t + \frac{ff}{tt} x.$

56. $TM = (TF + FM = \frac{fz}{m} + \frac{fz}{t} =) \; \frac{fzv}{tm} = \frac{zv}{x} = \frac{cc}{x}$

$tm = (Ct + Cm = \frac{fc}{n} + \frac{fn}{c} =) \; \frac{fzv}{cn} = \frac{zv}{y} = \frac{cc}{y}.$

57. $PM = \pi = \left(\frac{FR \times TM}{FT} =\right) \; \frac{c}{t}\sqrt{zv} = \frac{c}{t}C = \frac{p}{c}C = \frac{c}{t}\sqrt{tt \backsim \varphi^2 x^2}$

$= \frac{c}{t}\sqrt{\overline{t + \varphi x} \times \overline{t - \varphi x}} = \frac{cc}{tt} \times \frac{R}{S} x = \frac{cR}{tS}\sqrt{ce \mp yy}$

$= \frac{c}{tt}\sqrt{t^4 - ffxx} = \frac{c}{t}\sqrt{cc + \gamma\gamma yy} = \frac{1}{t}\sqrt{c^4 + ffyy}$

$= \frac{c}{t}\sqrt{2tz \mp zz} = \sqrt{2pz \mp \frac{p}{t} zz}.$

$Pm = \pi' = \left(\frac{PM \times Pd}{DM} =\right) \; \frac{t}{c}\sqrt{zv} = \frac{t}{c}C = \frac{c}{p}C = \frac{t}{c}\sqrt{tt \backsim \varphi\varphi xx}$

$= \frac{t}{c}\sqrt{\overline{t + \varphi x} \times \overline{t - qx}} = \frac{1}{c}\sqrt{t^4 \backsim ffxx} = \frac{t}{c}\sqrt{cc + \gamma\gamma yy}$

$= \frac{t}{cc}\sqrt{c^4 + ffyy} = \frac{t}{c}\sqrt{2tz \mp zz} = \sqrt{\frac{2rt}{p} z \mp \frac{t}{p} zz}.$

58. $Mm = (Pm \mp PM = \frac{t}{c}C \mp \frac{c}{t}C =) \; \frac{tt \mp cc}{ct} C = \frac{ff}{ct}\sqrt{zv}.$

$= \frac{ff}{ct}\sqrt{tt \backsim \varphi\varphi xx}.$

59. $PT = \tau = \left(\frac{PD \times PM}{DM} =\right) \; \frac{n}{m}C = \sqrt{\frac{zv - cc}{tt - zv} \times zv} = \frac{tn}{fx}C.$

$= \frac{ty}{cx}C = \frac{ty}{cx}\sqrt{tt \backsim \varphi\varphi xx} = \frac{\sqrt{\overline{zv - cc} \times zv}}{t \mp z}.$

$Pt = \tau' = \left(\frac{PT \times Pd}{DT} =\right) \; \frac{m}{n}C = \sqrt{\frac{tt - zv}{zv - cc} \times vz} = \frac{fx}{tn}C$

$= \frac{cx}{ty}C = \frac{cx}{ty}\sqrt{tt - \varphi\varphi xx}.$

60. TN

60. $\text{TN} = \left(\frac{\text{PT} \times \text{TA}}{\text{DT}} = \frac{n\,\text{C}}{m} \times \frac{t}{m} \times \overline{f \varpi m} \times \frac{fm}{tnn} =\right) \frac{f\,\text{C}}{mn} \times \overline{f \varpi m} = \frac{c\,\text{C}}{xy} \times \overline{t \varpi x}$

$\text{T}n = \left(\frac{\text{PT} \times \text{T}a}{\text{DT}} = \frac{n\,\text{C}}{m} \times \frac{t}{m} \times \overline{f + m} \times \frac{fm}{tnn} =\right) \frac{f\,\text{C}}{mn} \times \overline{f + m} = \frac{c\,\text{C}}{xy} \times \overline{t + x}$

61. $\text{PN} = \left(\frac{\text{AD} \times \text{PT}}{\text{DT}} = \frac{t}{f} \times \overline{f \varpi m} \times \frac{n\,\text{C}}{m} \times \frac{fm}{tnn} =\right) \frac{\text{C}}{n} \times \overline{f \varpi m} = \frac{c\,\text{C}}{ty} \times \overline{t \varpi x}$

$\text{P}n = \left(\frac{a\,\text{D} \times \text{PT}}{\text{DT}} = \frac{t}{f} \times \overline{f + m} \times \frac{n\,\text{C}}{m} \times \frac{fm}{tnn} =\right) \frac{\text{C}}{n} \times \overline{f + m} = \frac{c\,\text{C}}{ty} \times \overline{t + x}$

62. $\text{PR} = \left(\frac{\text{PT} \times \text{FM}}{\text{TM}} =\right) \frac{nz}{\text{C}} = n\sqrt{\frac{z}{v}} = \sqrt{\frac{z}{v} \times \overline{zv - cc}} = \frac{f}{c} y \times \sqrt{\frac{z}{v}}$

$$= \frac{f}{c} y \times \sqrt{\frac{t - \varphi x}{t + \varphi x}} = \frac{f}{cc} \times \lambda y$$

$\text{P}r = \left(\frac{\text{PT} \times f\,\text{M}}{\text{TM}} =\right) \frac{nv}{\text{C}} = n\sqrt{\frac{v}{z}} = \sqrt{\frac{v}{z} \times \overline{zv - cc}} = \frac{f}{c} \times y\sqrt{\frac{v}{z}} = \frac{f}{c} y \times \sqrt{\frac{t + \varphi x}{t - \varphi x}}$

63. $\text{T}\varrho = \left(\frac{\text{PT} \times \text{TC}}{\text{TM}} =\right) \frac{ntt}{m\,\text{C}} = \frac{tt}{t - z} \sqrt{\frac{zv - c\varrho}{vz}} = \frac{fytt}{mcc} = \frac{yt^3}{xcc}$

64. $\text{P}\varrho = (\text{T}\varrho \mp \text{TP} = \frac{ntt}{m\,\text{C}} \mp \frac{n\,\text{C}}{m} \times) \frac{n}{m\text{C}} \times tt \mp \text{CC} = \frac{n}{m\text{C}} \times mm = \frac{nm}{\text{C}}.$

65. $\text{T}t = \left(\frac{\text{PT} \times \text{C}\,t}{\text{PD}} = \frac{n\,\text{C}}{m} \times \frac{cf}{n} \times \frac{f}{cn} =\right) \frac{ff\,\text{C}}{mn} = ff\sqrt{\frac{zv}{tt - zv \times zv - cc}}$

$$= \frac{tc}{xy}\text{C} = \frac{tc}{xy}\sqrt{tt - \frac{ff}{tt}xx}$$

66. $\text{N}n = \left(\frac{\text{PT} \times \text{A}\,a}{\text{DT}} =\right) \frac{2f\,\text{C}}{cn} = \frac{2c}{y}\,\text{C} = \frac{2c}{y}\sqrt{zv} = 2f\sqrt{\frac{zv}{zv - cc}}$

From C and F, draw Cγ and Fε, perpendicular to Pf
Put P$\varepsilon = h$; Σ = *co*-sine of the $\angle$ AfP; R = tabular radius.

Then $f\gamma = \left(\frac{\text{C}f \times f\,\text{D}}{\text{P}f} = \frac{tv - cc}{f} =\right) \frac{tv - cc}{v} = \left(\frac{\Sigma}{\text{R}} \times \text{C}f\right) \frac{\Sigma f}{\text{R}}$

And $\text{P}f = v = \frac{cc}{t - \frac{\Sigma}{\text{R}}f} = \frac{tt - ff}{t - \frac{\Sigma}{\text{R}f}}$

Pι

$$P\iota = h = \left(\frac{\pm zz + vv \mp 4ff}{2v} = \frac{\pm TT \mp ff}{v} = \frac{\pm 2bt \mp vz}{v} =\right)$$

$$= \pm \frac{2pt}{v} \mp z = \pm \frac{z+v}{v} p \mp z.$$

Hence $p = \frac{\pm h \mp z}{z+v} \times v$; $v = \frac{pz}{\mp b \pm z - p}$; $z = \frac{p-h}{v-p} \times v$

Therefore p is leſs than, equal to, or greater than $\pm h \mp z$, in the Ellipſis, Parabola, or Hyperbola.

67. Draw $m\mu$, making the $\angle$ PMμ = $\angle$ PCM, and cutting PC in μ.

Then $P\mu = \left(\frac{PM \times Pm}{PC} =\right) \frac{CC}{T} = \frac{zv}{\sqrt{tt \pm cc \mp zv}} = \frac{tt - \phi\phi xx}{\sqrt{cc + \phi\phi xx}}$

$$= P = \tfrac{1}{2} \text{ parameter to PC.}$$

68. Let FΦ be an ordinate to the axis Aa. at the focus F. and ΦG a tangent to the curve in Φ, meeting Aa, BC, AN, an, in G, g, S, s. Then

$$F\Phi = \sqrt{\frac{cc}{tt} \times AFa} = \sqrt{\frac{cc}{tt} \times cc} =) \frac{cc}{t} = \frac{pt}{t} = p$$

$$= \tfrac{1}{2} \text{ parameter to A}a.$$

69. $CG = \frac{tt}{f} = \frac{ff \pm cc}{f} = \frac{tt}{\sqrt{tt - cc}} = \frac{tt}{\sqrt{t \times t - p}}$

70. $FG = (CG \backsim CF = \frac{tt}{f} \backsim f = \frac{tt \backsim ff}{f} = \frac{cc}{f} = \frac{pt}{f}.$

71. Draw PH parallel to Aa; and GH perpendicular to Aa, meeting, PH, PT, in H, h; then

$$PH = DG = (CG \backsim DC =) \frac{tt}{f} \backsim x = \frac{t}{f} z = \frac{t}{f} \times PF.$$

72. TG

72. $\text{TG} = (\text{CT} \backsim \text{CG} = \frac{ft}{m} \backsim \frac{tt}{f} = \frac{t}{m} \times \overline{f \backsim x} =) \frac{t}{t-z} \times \frac{\pm tz \mp cc}{f} = \frac{tt}{fx} \times \overline{f \backsim x}.$

73. $\text{G}h = \left(\frac{\text{PD} \times \text{TG}}{\text{DT}} = \frac{cn}{f} \times \frac{t}{m} \times \overline{f \backsim x} \times \frac{fm}{tnn} = \frac{\text{C}}{n} \times \overline{f \backsim x} =\right)$

$$\frac{cc}{fy} \times \overline{f \backsim x} = \frac{c}{\sqrt{zv - cc}} \times \frac{tz - cc}{f}.$$

74. $\text{P}h = \left(\frac{\text{TP} \times \text{DG}}{\text{DT}} =\right) \frac{z}{n}\text{C} = \frac{cz}{fy}\text{C} = z \times \sqrt{\frac{zv}{zv - cc}} = \frac{cz}{fy} \sqrt{zv}.$

75. $\text{R}h = (\text{P}h - \text{PR} =) \frac{z\text{C}}{n} - \frac{zn}{\text{C}} = \frac{z}{n\text{C}} \times \overline{\text{CC} - nn} =)$

$$\frac{zcc}{n\text{C}} = \frac{zcc}{\sqrt{zv - cc} + zv} = \frac{zc^3}{fy\text{C}}$$

76. $\text{F}h = \left(\sqrt{\overline{\text{FR}}^2 + \overline{\text{R}h}^2} =\right) \frac{cz}{n} = \frac{cz}{\sqrt{zv - cc}} = \frac{ccz}{fy} = \frac{ptz}{fy}.$

77. Let DΣ be any Ordinate to the axe Aa, cutting the curve in Σ, and the focal tangent ΦG in σ;

Then $\text{D}\sigma = \left(\frac{\text{F}\Phi + \text{DG}}{\text{FG}} = \frac{cc}{t} \times \frac{tz}{f} \times \frac{f}{cc} =\right) z = \text{FP} = \text{F}\Sigma.$

78. Therefore AS = AF; as = aF; Cg = CA; by ſim. $\triangle$s.

79. $\overline{\text{DF}}^2 = (\overline{\text{D}\sigma}^2 - \overline{\text{D}\Sigma}^2 = \overline{\text{D}\sigma + \text{D}\Sigma} \times \overline{\text{D}\sigma - \text{D}\Sigma} =) \text{P}\sigma \times \sigma\Sigma.$

79. Let the tangents PN, pL, to the oppoſite vertices P, p, cut the tangents AN, an, to the oppoſite vertices A, a, in N, n, L, l.
Then Pn = pL; an = AL; PN = pl; AN al.
For the Trapezia's PCan, pCAL, are ſimilar and equal;
And ſo are the Trapezia's PCAN, pCal.

81. FF × Mf = $\left(\frac{f}{t} \times vz =\right)$ Pf × MF = $\frac{f}{t}$ × FP f.

82. CEQ = $(xy =)$ CDP = dCD = DM × dm.

83. AN × aD = $\frac{ctn}{f}$ = an × AD = AC × PD = Dal = DAL.

84. RPr = nn = TPϱ.

85. RTr = $\left(\frac{nn}{mm} \times tt = \frac{tt - xx}{xx} \times tt =\right)$ ATa.

86. RTf = $\left(\frac{tnz}{mc} \times \frac{fv}{m} = \frac{ft}{m} \times \frac{nc}{m} =\right)$ CTP = rTF.

87. NTn = $\left(\frac{ffcc}{mm\,nn} \times \overline{ff \backsim mm} =\right) \frac{ffcc}{mm}$ = PTf.

88. PN × CB = $\left(\overline{t \backsim x} = \frac{fc}{tn}c =\right)$ AN × CQ.

89. CAV = $\left(t \times \frac{ty}{x} = \frac{tt}{x} \times y =\right)$ CT × PD.

90. PRh = $\left(\frac{cczz}{cc} =\right) \overline{FR}^2$.

Hence Fh is perpendicular to FP.

91. FMf = $\left(\frac{fz}{t} \times \frac{fv}{t} =\right) \frac{ff}{tt} \times zv = \frac{ff}{tt} \times cc = \frac{ff}{tt} \times \overline{CQ}^2$

92. AMa = $\left(tt \backsim \frac{ffmm}{tt} =\right) cc \times \frac{ff}{tt} \times zv$

= $\overline{CB}^2 + \frac{ff}{tt} \times \overline{CQ}^2 = \overline{CB}^2$ + FMf.

93.

93. $\frac{\overline{PN}^2}{\overline{TN}^2}=\frac{xx}{tt}=\frac{\overline{CD}^2}{\overline{AC}^2}=\frac{\overline{AN}^2}{\overline{VN}^2}=\frac{\overline{CA}^2}{\overline{CT}^2}=\frac{AD.a}{AT.a}=\frac{AD.a}{RT.r}=\frac{\overline{AD}^2}{\overline{AT}^2}=\frac{\overline{aD}^2}{\overline{aT}^2}=\frac{CDT}{ATa}$.

94. $\frac{FT}{CT}=\frac{z}{t}=\frac{FP}{CA}=\frac{FR}{C\varrho}$. Sim. $\triangle$s TFR, TCϱ.

And $\frac{AD}{DT}=\frac{x}{t+x}=\frac{CD}{aD}=\frac{AC}{aT}$.

95. $\frac{fT}{CT}=\frac{v}{t}=\frac{fP}{AC}=\frac{fr}{G\varrho}$. And $\frac{AT}{DT}=\frac{t}{t+x}=\frac{AC}{aD}=\frac{CT}{aT}$.

96. $\frac{PM}{Pm}=\frac{cc}{tt}=\frac{p}{t}=\frac{\overline{BC}^2}{\overline{AC}^2}$. And $\frac{Pm}{fm}=\frac{t}{f}=\frac{AC}{CF}$.

97. $\frac{FM.f}{\overline{PM}^2}=\frac{ff}{cc}=\frac{\overline{t+c}\times\overline{t-c}}{cc}=\left(\frac{tt-tp}{tp}=\right)\frac{t-p}{p}=\frac{\overline{CF}^2}{\overline{CB}^2}$.

98. Let PM, the perpendicular to the tangent PT, cut the axis Aa in M; and fP, produced, cut FR in φ.
Then will PM biſect the angle FPf.
For PF $\times$ Mf = Pf $\times$ MF.

99. And the angle FPT is equal to the angle fPt.
For $\angle$TPF + $\angle$FPM = $\angle t$Pf + $\angle f$PM.

100. Therefore PT will biſect the angle FPφ.
For $\angle\varphi$PT = ($\angle f$Pt =) $\angle$FPT.

101. Conſequently Pφ = PF. And Rφ = RF.

102. A circumference of a circle deſcribed from t, with the radius tN, will cut the axis Aa in the focii F, f.
For TN $\times$ Tn = TF $\times$ Tf.

103. A circumference of a circle deſcribed from C, with the radius CA, will cut the tangent PT in R, *r*.

Whence the perpendiculars RF, *rf*, to that tangent, will cut the axis A*a* in the focii F, *f*.

For TR × T*r* = TA × T*a*.

104. A circumference deſcribed from B, with AC, in the Ellipſis, or from C, with AB in the Hyperbola, will cut the axis A*a*, in the focii F, *f*.

For, in the Ellipſis, $tt = cc + ff$, or $\overline{AC}^2 = (\overline{BC}^2 + \overline{CF}^2 =) \overline{BF}^2$.

And in the Hyperbola, $ff = tt + cc$, or $\overline{CF}^2 = (\overline{AC}^2 + \overline{BC}^2 =) \overline{AB}^2$.

105. Let CQ produced, cut PF, P*f*, in z, *x*; draw *mz*, *mx*, and MZ, MX, parallel to them, cutting PF, P*f* in Z, X.

Then P*x* = CR = CA = P*z* = *t*.

106. Hence ∠P*x*z = ∠P*z*x; and ∠*mx*z = ∠*mz*x.

For P*m* is perpendicular to z*x*.

Conſequently, the angles P*z*m, P*x*m; PZM, PXM are equal.

107. And the triangles P*z*m, P*x*m, are ſimilar and equal:

And ſo are the triangles PZM, PXM.

Conſequently, the trapezias P*z*m*x*, PZMX, are ſimilar.

108. Let CR, C*r*, cut PF, P*f*, in K, *k*.

Then $CK = \left(\frac{CF \times Pf}{Ff} =\right) \frac{1}{2} Pf = Pk = kr$.

And $Ck = \left(\frac{Cf \times PF}{Ff} =\right) \frac{1}{2} PF = PK = KR$.

109. Alſo $PX = PZ = \left(\frac{PM}{Pm} \times Pz = \frac{cc}{t} \times \frac{c}{tc} \times t =\right) \frac{cc}{t} = \left(\frac{pt}{t} =\right) p$.

110. The

110. The Trapezias FGbR, PDFR, frtC, are similar, and consequently their corresponding parts are proportional.

That is, $\left\{\begin{matrix}\frac{FG}{PD}=\frac{G\,b}{DF}=\frac{b\,R}{FR}=\frac{RF}{RP}\\ \overline{fr}=\overline{rt}=\overline{tC}=\overline{Cf}\end{matrix}\right\}$

For the triangles R$F b$, RPF, and FbG, FPD, are similar.

111. The Trapezias CϱPD, tCMP are similar, and consequently their corresponding parts are proportional.

That is, $\frac{C\varrho}{tC}=\frac{\varrho P}{CM}=\frac{PD}{PM}=\frac{DC}{Pt}$.

112. And CR, Cr, are parallel to Pf, PF, and equal to CA.

For RTf = CTP = rTF.

113. Let Σ' = sine of the $\angle tPf$, or TPF; R = tabular radius.

Then $\frac{R}{\Sigma'}=\left(\frac{Pf}{fr}=\right)\frac{C}{c}=\frac{\sqrt{zv}}{c}$.

Put Σ = sine of the $\angle$PCQ, made by any diameter and its ordinate.

Then $\frac{TT}{CC}=\frac{1}{2}ff\pm\sqrt{\frac{1}{4}f^4\mathcal{S}\frac{r^2t^2c^2}{\Sigma^2}}$; $\frac{tt}{cc}=\frac{1}{2}FF\pm\sqrt{\frac{1}{4}F^4\mathcal{S}\frac{T^2C^2\Sigma^2}{rr}}$.

114. Let the parallels PFΠ, Cβr, $pf\pi$ be drawn, cutting the curve in Π, β, π; and ordinately applied to some diameter (2τ), whose parameter is 2π, and semiconjugate Cβ = χ, to which Pπ is ordinately applied at δ.

Then Cδ = $\left(\frac{PF+\pi f}{2}=\frac{PF+F\Pi}{2}=\right)\frac{1}{2}z+\frac{1}{2}z'=\frac{1}{2}P\Pi=\left(\frac{\overline{C\beta}^2}{Cr}=\right)\frac{\overline{C\beta}^2}{AC}=\frac{\chi\chi}{t}=\frac{\pi\tau}{t}$.

115. $\overline{C\beta}^2=\left(\frac{1}{2}AC\times P\Pi=\right)\frac{1}{2}t\times\overline{z+z'}=\left(\overline{AC}^2\times\frac{PF\Pi}{AFa}=\right)\frac{tt}{cc}zz'=\frac{t}{p}zz'$.

116. $\frac{FP}{F\Pi}=\frac{z}{z'}=\frac{FD}{F\Delta}=\frac{tz-cc}{cc-tz'}=\frac{z-p}{p-z'}$.

117. $PF\Pi=\frac{pzz}{2z-p}=\frac{1}{2}p\times\overline{z+z'}=\frac{1}{2}p\times P\Pi$.

118. $P\Pi=z+z'=\frac{2zz}{2z-p}=\frac{2\pi\tau}{t}=\frac{\tau}{t}\times 2\pi$.

$\overline{CB}^2$

$$\overline{CB}^2 = cc = pt = \pm tt \mp ff = \pm \overline{t \mp f} \times \overline{t + f} = AF\,a = VAU = tC\,d = CFG$$
$$= PM \times C\varrho = FR \times fr = AN \times an = al \times AL = AC \times F\Phi = tt \times \frac{PM}{P\,m}$$
$$= tt \times \frac{DM}{DC} = tt \times \frac{dC}{d\,m} = tt \times \frac{\overline{PM}^2}{\overline{CQ}^2} = tt \times \frac{\overline{PD}^2}{AD\,a} = tt \times \frac{\overline{PD}^2}{TDC} = ff \times \frac{PD}{C\,m}$$
$$= \left|\frac{CQ \times C\varrho}{AC}\right|^2 = \left|\frac{AN \times CQ}{PN}\right|^2 = AM\,a - FM\,f = FP\,f - RP\,r = ff \times \frac{t}{t - \phi}$$
$$= TT + CC - tt = \frac{tt}{xx} \times \overline{zv + xx - tt} = \tfrac{1}{2}zv \pm \sqrt{\tfrac{1}{4}z^2v^2 - ffyy}.$$

$$\overline{CQ}^2 = CC = P \times T = MP\,m = NP\,n = TP\,t = FP\,f = CP\,\eta = L\,p\,l$$
$$= PN \times p\,L = CD \times TM = PD \times t\,m = \frac{tt}{cc} \times \overline{PM}^2 = \frac{cc}{tt} \times \overline{P\,m}^2 = \frac{cc}{ff} \times \overline{f\,m}^2$$
$$= \frac{tt}{ff} \times FM\,f = \left|\frac{ct}{ff} \times M\,m\right|^2 = \left|\frac{ACB}{C\varrho}\right|^2 = \overline{PM}^2 + FM\,f = \overline{BC}^2 + RP\,r$$
$$= tt + cc - TT = tt - \frac{ff}{tt}xx = \frac{tt}{cc}yy + \frac{cc}{tt}xx = \frac{DC}{DT} \times \overline{PT}^2.$$

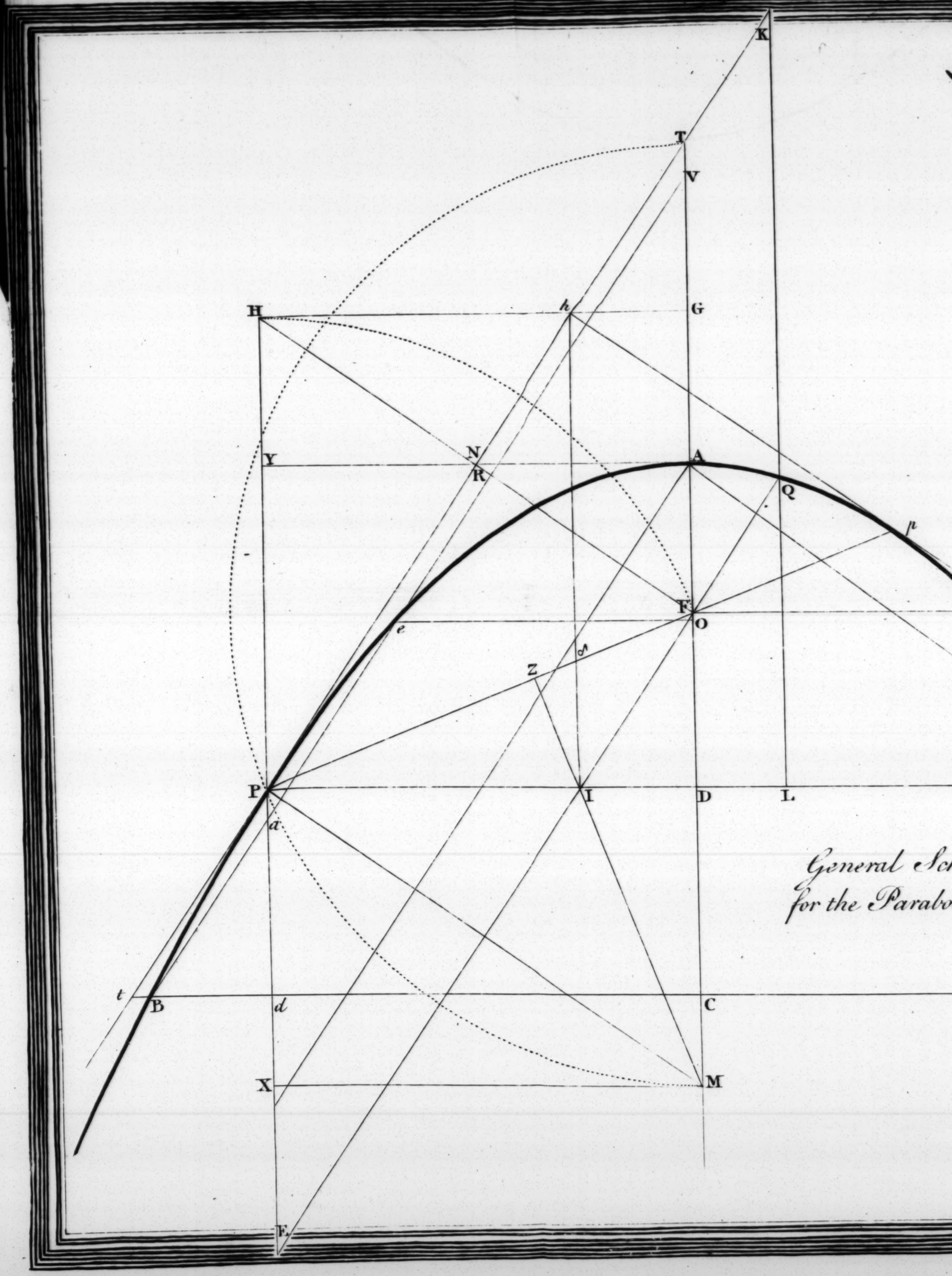
$$\overline{CF}^2 = ff = tt - cc = tt - tp = \frac{cc}{p} \times \overline{t - p} = \frac{tt}{xx} \times mm = P\varrho \times Tt$$
$$= \frac{tt}{CC} \times FM\,f = \frac{cc}{CC} \times \overline{f\,m}^2 = \frac{mm}{CC} \times FT\,f = \frac{mm}{CC} \times NT\,n.$$

The ſemi-parameter (p) to the greater axis (Aa) is equal to

$$F\Phi = PZ = \frac{AF\,a}{AC} = \frac{VaU}{AC} = \frac{CFG}{AC} = \frac{tC\,d}{AC} = \frac{PF\Pi}{\frac{1}{2}P\Pi} = \frac{PM \times C\varrho}{AC}$$
$$= \frac{FR \times fr}{AC} = \frac{AN \times an}{AC} = \frac{AC \times PM}{P\,m} = \frac{AC \times DM}{DC} = \frac{AC \times d\,C}{d\,m} = \frac{BC \times PM}{CQ}$$
$$= \frac{BC \times CQ}{P\,m} = \frac{\overline{BC}^2}{AC} = \frac{cc}{t} = \frac{ttyy}{sx} = \frac{\pm tt \mp ff}{t} = \frac{cc - yy}{xx} \times t$$
$$= \frac{CC + TT - tt}{t} = \frac{CC + xx - tt}{xx} \times t = \frac{2t - z}{tz} \times \overline{FR}^2 = \frac{2t - v}{tv} \times \overline{fr}^2.$$

The ſemi-parameter (P) to any diameter (Pp) is equal to

$$P\eta = \frac{\overline{CQ}^2}{PC} = \frac{CC}{T} = \frac{MP\,m}{PC} = \frac{NP\,n}{PC} = \frac{TP\,t}{PC} = \frac{FP\,f}{PC} = \frac{L\,p\,l}{PC} = \frac{PN \times p\,L}{PC}$$
$$= \frac{CD \times TM}{PC} = \frac{PD \times t\,m}{PC} = \frac{PT \times PN}{PV} = \frac{tt}{cc} \times \frac{\overline{PM}^2}{PC} = \frac{cc}{tt} \times \frac{\overline{P\,m}^2}{PC}$$
$$= \frac{cc}{ff} \times \frac{\overline{f\,m}^2}{PC} = \frac{tt}{ff} \times \frac{PM\,f}{PC} = \frac{tt + cc - TT}{T}.$$ [See TAB. XIV, XV.]



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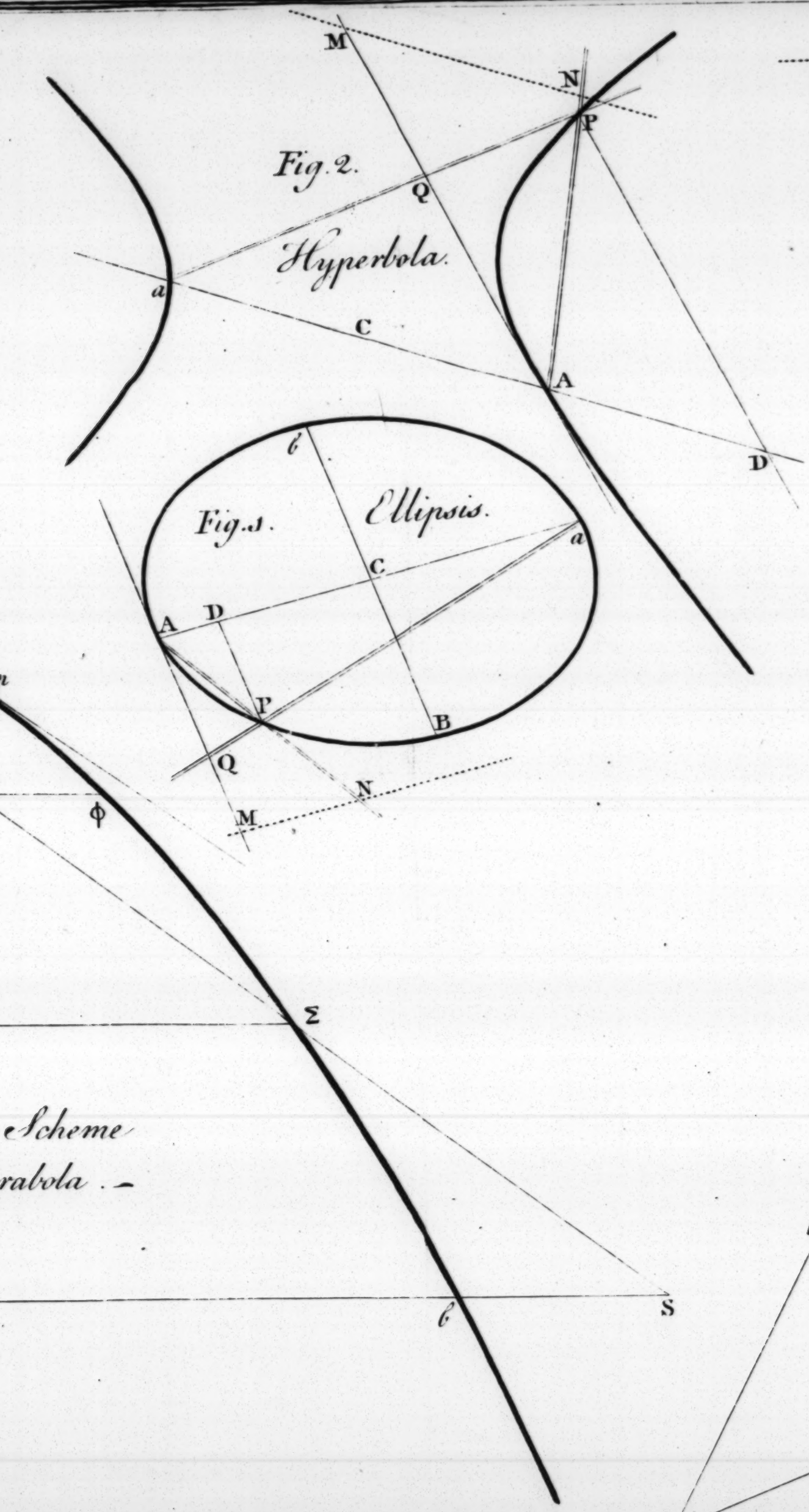


Fig. 2. Hyperbola.

Fig. 3. Ellipsis.

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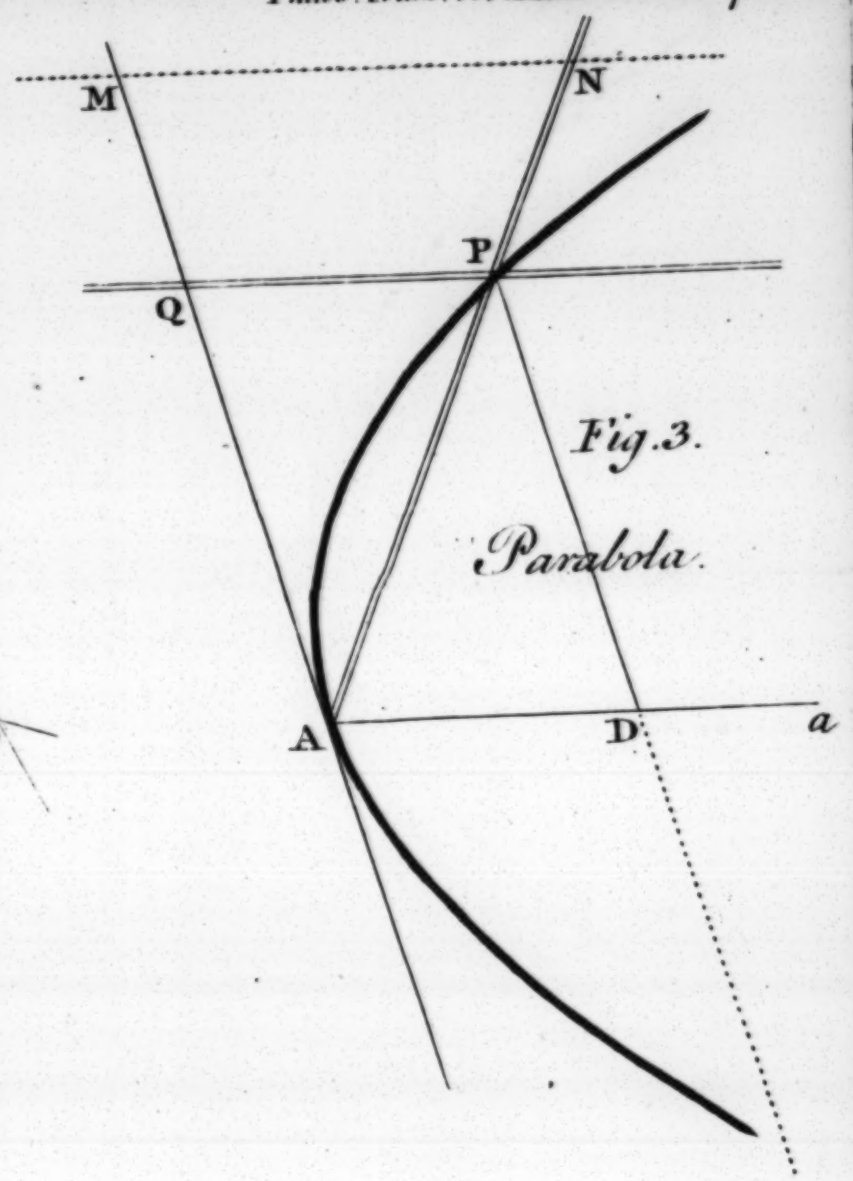


Fig. 3. Parabola.

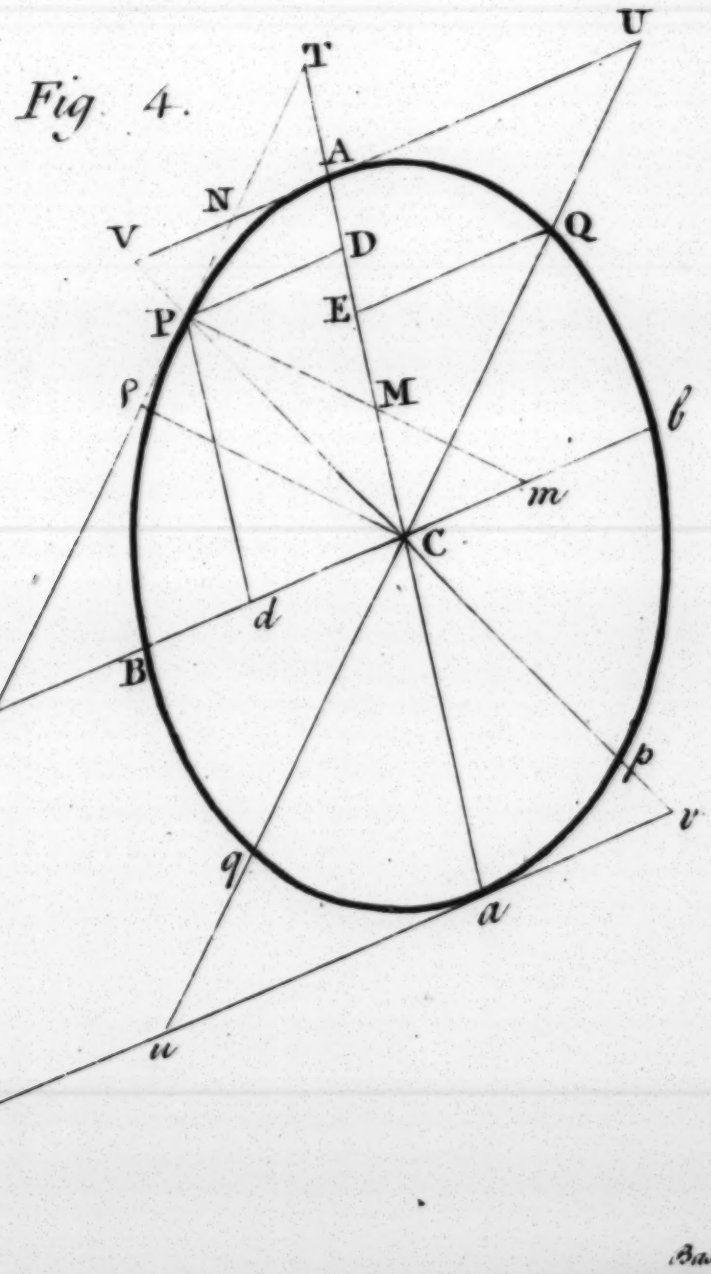
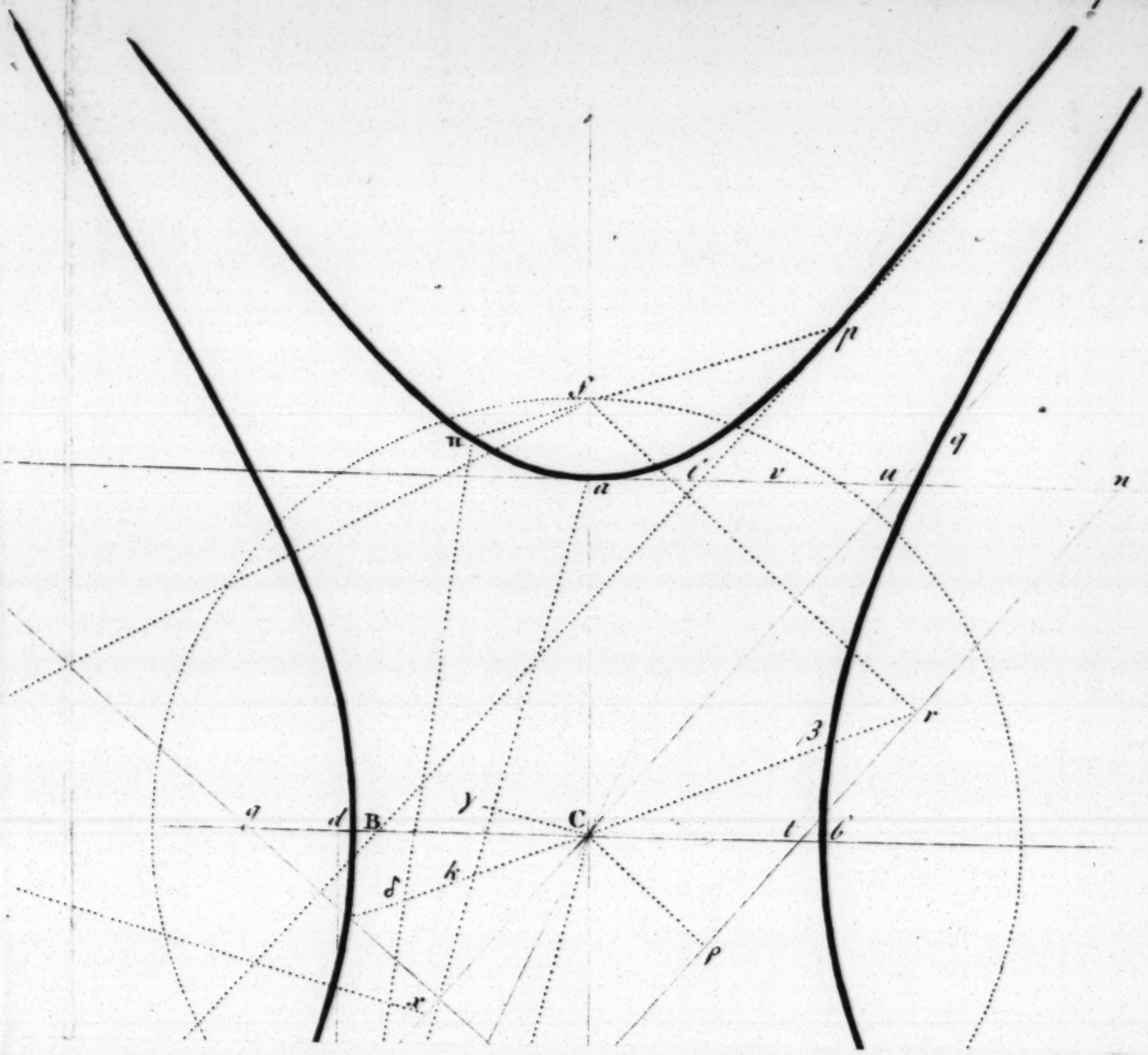


Fig. 4.

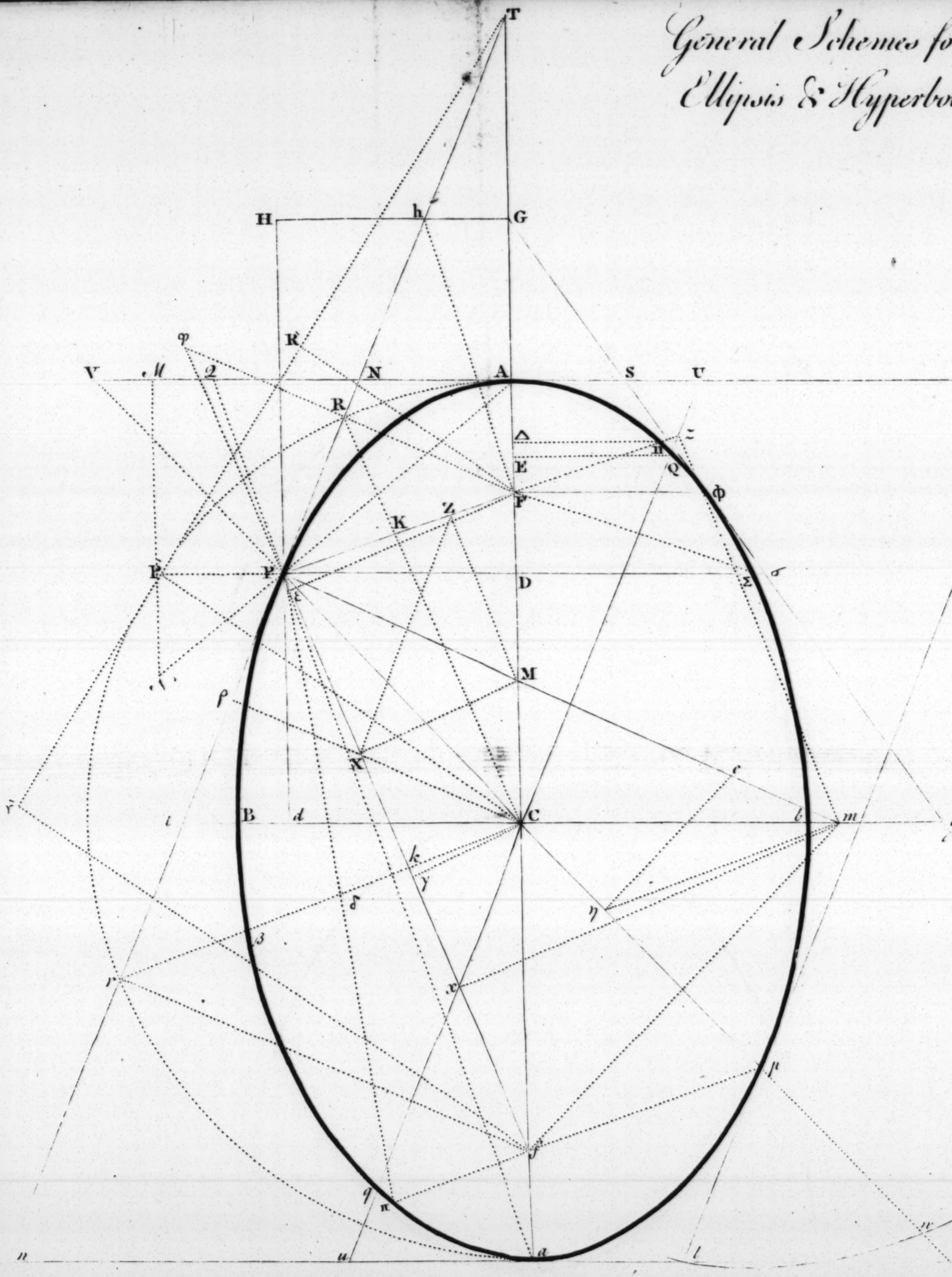
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Philos. Trans. Vol. LXIII. Tab. XV. p. 360.

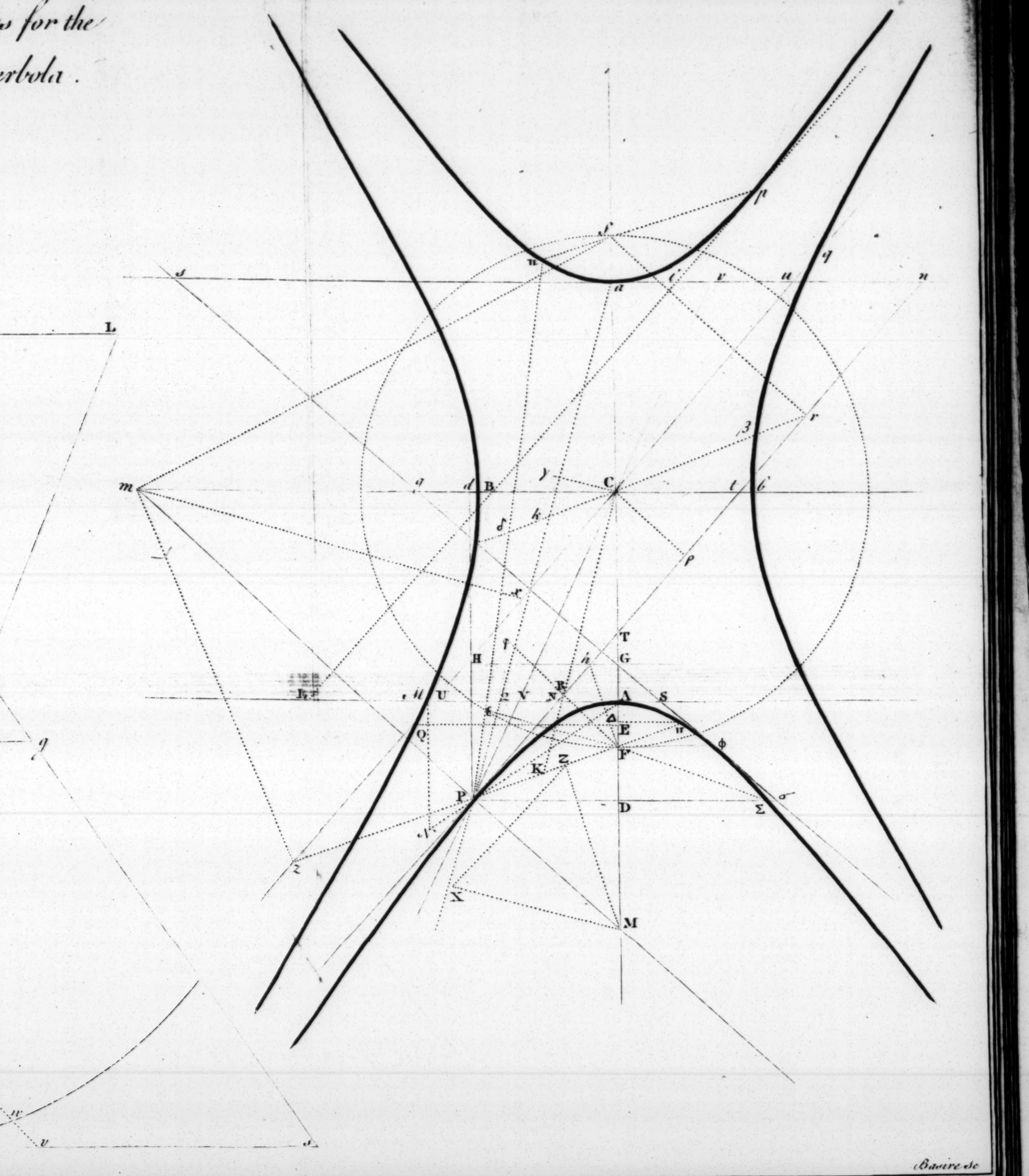


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XXXVII. *An Eſſay, towards elucidating the Hiſtory of the Sea-Anemonies, by Abbé* Dicquemare, *Member of ſeveral Academies, Profeſſor of Experimental Philoſophy, &c. at* Havre de Grace. *Communicated by* Matthew Maty, M. D. *Sec. R. S. Tranſlated from the* French.

Read Feb. 11, and April 29, 1773.

WHatever is relative to animals, their mode of being, growth, and decreaſe, propagation, ſtrength, actions, diſtempers, food, length of life, and even the phænomena obſerved at their death, are ſo many objects intereſting to man. If his moral qualities bear no analogy with theirs, his natural conſtitution admits of ſimilarities between them.

Memoire pour ſervir à l'hiſtoire des Anemones de Mer; par Mr. l'Abbé Dicquemare, *de pluſieurs Académies Royales, Profeſſeur de Phyſique Expérimentale, au* Havre de Grace.

TOUT ce qui a rapport aux animaux, leur maniere d'être, les accroiſſemens et décroiſſemens qu'ils éprouvent, leur génération, leur force, leurs actions, leurs maladies, leur nourriture, la durée de leur vie, les phénomenes qu'ils nous préſentent même à la mort, ſont autant d'objets qui doivent intereſſer l'homme. Si ſon être moral n'offre avec eux aucune analogie, ſa conſtitution phyſique permet des ſimilitudes.

Comparative anatomy, and experiments made upon living animals, have unravelled to us part of their vital, natural, and animal faculties. Among the laſt, thoſe which depend upon the organic diſpoſition of the parts, and which perhaps are the principle of many more functions, might receive new lights from obſervations to be made upon ſuch animals, as ſeem to deviate moſt from our mode of being. Surprized by the phænomena theſe animals afford, we pry with eager looks into the ſecrets of nature; there we perceive effects, which ſhew us, that the inmoſt conformation of ſome particular animals not tallying with our former notions, we muſt renounce, as groundleſs prepoſſeſſions, ſuch laws as we had been too haſty in laying down as univerſal. Thoſe effects, and the method of obſerving them, which ought, ſeemingly, upon a firſt inſpection, to enable us,

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L'Anatomie comparée, et les expériences faites ſur les animaux vivans, nous ont dévoilé une partie des fonctions vitales, naturelles, et animales. Parmi ces dernieres, celles qui dépendent de la diſpoſition organique des parties et qui ſont peut-être le principe de beaucoup d'autres, pourroient recevoir une nouvelle lumiere des obſervations à faire ſur les animaux, qui ſemblent s'éloigner le plus de notre maniere d'être. Surpris par les phénomenes qu'ils nous préſentent, on fouille avec avidité dans le ſein de la nature; on y aperçoit des effets qui nous aprenent que la conformation la plus intime de certains animaux ne s'accordant pas avec nos anciennes idées, nous devons rejetter comme préjugés, des loix qu' on s'étoit trop preſſé de généraliſer. Ces effets, et la maniere de les obſerver, qui ſemblent au premier coup d'oeuil devoir nous raprocher peu à peu de la cauſe, ſoutenir nos eſpérances, flater notre

us, by degrees, to inveſtigate the cauſe, ſupport our hopes, and flatter our vanity; are nevertheleſs meant to ſhew us that the marvellous works of nature far exceed what we know, or even are able to know of them. Convinced of that truth, and in order to improve my cuſtomary walks by the ſea ſide to ſome uſeful purpoſe, I beſtowed a particular attention, in the month of November 1771, upon the *Sea-Anemonies*, a moſt ſingular ſpecies of animals, on which naturaliſts have hitherto made leſs ample and accurate obſervations. Some of thoſe writers have even confined themſelves to mere conjectures. My firſt ſucceſs in theſe diſcoveries ſoon turned theſe amuſements of mine into a long ſtudy, much more laborious than that made in a library.

There

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notre vanité, n'en paroiſſent pas moins deſtinés à nous faire voir que les merveilles de la nature ſont bien au deſſus de ce que nous connoiſſons, et ſans doute de ce que nous pouvons connoitre. Pénétré de ces vérités, et dans la vue de répandre une ſorte d'intéreſt dans les délaſſemens que j'ai coutume de prendre au bord de la mer, je jettai les yeux dès le mois de Novembre 1771, ſur les *Anémones de Mer*, animaux ſinguliers ſur leſquels les naturaliſtes n'avoient fait juſque là que des obſervations moins préciſes et moins étendues; ſouvent même s'en ſont-ils tenus à de ſimples conjectures. Mes premiers ſuccès changerent bientot ces récréations en un travail long et plus pénible que ceux du cabinet.

There is great confuſion in the deſcriptions which naturaliſts have given of theſe animals, and no leſs in the names beſtowed upon, and the diviſions or claſſes aſſigned to, them. Some have called them *Sea-Nettle* (Urticæ Marinæ), though theſe animals are not prickly, as ſome of the wandering *Nettles* are, upon which I have, for ſome time paſt, made obſervations which I intend to purſue. Other writers have called them *Sea-Anemonies*. This eſſay, together with the plates annexed, will ſatisfy the reader as to the name I have preferred. It would be needleſs to inſiſt longer about names.

The Sea-Anemonies, found on the coaſt of the Havre, ſeem to me to conſtitute three different ſpecies. Thoſe which I put in the firſt claſs (becauſe in certain poſitions they reſemble moſt the flower known by the name of Anemone) cling or adhere to rocks and ſtones, and are often found in the holes that chance

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Il regne une grande confuſion dans les deſcriptions que nous ont donné de ces animaux les naturaliſtes, dans les noms qu'ils leur ont impoſés, et dans les diviſions qu'on en a faites. Quelques uns les nomment Orties Marines *(Urticæ Marinæ)* quoiqu'ils ne piquent point comme quelques orties errantes, ſur leſquelles j'ai fait depuis longtemps des obſervations que je me propoſe de continuer. D'autres leur ont donné le nom d'*Anémones de Mer*. Ce mémoire, et les figures qui l'accompagnent pourront juſtifier le choix que j'ai fait de ce nom. Il eſt inutile d'étendre plus loin cette nomenclature.

Les rivages du Havre offrent des Anémones de Mer, qui m'ont paru former trois eſpeces. Celles que je regarde comme de la premiere (parce que dans certaines poſitions elles reſſemblent plus parfaitement à la fleur qui porte le nom d'Anémone) s'attache

chance to be in them, and ſeem to like the ſurface of the water. The outward ſhape of the body of this animal, when it contracts itſelf, is much like a truncated cone [Tab. XVI. fig. 1.], with its baſis fixed and ſtrongly clinging to the rock. Its upper part is terminated with a hollow. This cone is often perpendicular to its baſis; ſometimes it lies in an oblique poſition to it, or the baſis ſpreads itſelf irregularly; ſo that from a round, it alters to an elliptical ſhape. Sometimes it imitates pretty exactly the incloſing out-leaves of anemonies, whilſt the limbs of the animal are not unlike the ſhag, or inner part of theſe flowers, fig. 2. At other times, it aſſumes the ſhape expreſſed by fig. 3. Indeed, theſe animals alter their forms ſo often, that it would be difficult, perhaps even impoſſible, to deſcribe them exactly. One part of their

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s'attache aux rochers, aux cailloux, ſouvent même dans les troux qui s'y trouvent, et ſe plait à la ſurface de l'eau. La forme extérieure du corps de l'animal, lorſqu'il eſt retiré ſur lui même aproche beaucoup de celle d'un cone tronqué, (Pl. XVI. Fig. 1.) dont la baſe eſt apliquée et fortement atachée contre les rochers, et la partie ſupérieure ſe termine par une convexité. Souvent ce cone eſt perpendiculaire à la baſe; quelquefois il eſt à ſon égard dans une poſition oblique, la baſe s'élargit irrégulierement, en ſorte que de ronde elle devient elliptique. On la voit quelquefois imiter aſſez bien le manteau d'une anemone, tandis que les membres de l'animal paroiſſent en être la pluche, Fig. 2. d'autres fois il prend la forme de la figure 3. D'ailleurs ces animaux varient ſi ſouvent leur forme, qu'il ſeroit dificile, peut-être même impoſſible, de les bien décrire. Une partie de leur

their body or limbs ſwells, at times, very conſiderably, at the expence of the reſt. The figures and the particular obſervations will ſupply what is wanting here. With regard to their colours, they vary amazingly. Every hue of purple, green, brown and violet is to be ſeen blended together. A great number of them are of one uniform colour; whilſt others are ſpotted either ſymetrically, as in ſtripes, or in an irregular, but always pleaſing, manner. Moſt of them have, round their baſis, a blue or white ſtreak, broader or narrower, which produces a ſort of ring. When many of theſe animals are put together at the bottom of a flattiſh and wide veſſel, the whole appears as a bed of Anemonies.

The Sea-Anemonies of the ſecond ſpecies are pretty nearly ſhaped out as thoſe of the firſt, but they are much larger. I have ſome by me, kept in ſea

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corps ou de leurs membres s'augmente quelquefois conſidérablement aux dépends des autres. Les figures et le détail des obſervations ſupléeront à ce qui manque ici. Quant à leurs couleurs elles varient étonnenment; toutes les nuances de pourpre, de verd, de brun, de violet y ſont employées et combinées. Un grand nombre ſont partout d'une même couleur, d'autres ont des taches ſimetriquement rangées formant des rayes, ou même diſtribuées irrégulierement, mais toujours d'une maniere agréable. Preſque toutes ont autour de leur baſe une bande bleüe ou blanche, plus ou moins étroite, qui y forme une bordure. Lorſqu'on en a beaucoup d'ouvertes dans le fond d'un vaſe plat et large, il ſemble d'un parterre d'anémones.

Les Anémones de Mer de la ſeconde eſpece ſont à peu près formées comme celles de la premiere, mais elles ſont beaucoup plus grandes. J'en conſerve dans l'eau de mer, dont les membres

ſea water, which are eighteen or twenty inches in circumference. Their cloak or outward ſkin is rough like ſhagreen, or full of little knobs. See Tab. XVI. fig. 10. and Tab. XVII. fig. 11. and 12. where they are ſhewn in the natural ſize. They remain in the ſand, ſticking to the looſe ſtones in it, and ſtretch out their limbs to the top, in order to lay hold of their prey, as ſoon as it touches the ſuperficies of the ſand. The flower of poppies is ſaid to be the plague and diſtreſs of painters, to repreſent exactly the variety and brilliancy of its colours; the ſame may be ſaid of the Sea-Anemonies of this larger ſpecies. The pureſt white, carmine, and ultramarine, would hardly be bright enough to paint them properly. The limbs of ſome of them are of a moderate or dim colour, at the ſame time that the cloak is made up of the brighteſt colours.

The third ſpecies ſeems to deviate a little more from the ſecond, than this from the firſt. Its body, not unlike for ſhape and colour to the ſtalk of a muſh-

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membres étendus forment une circonférence de 18 à 20 pouces; leur robe (la peau extérieure) eſt chagrinée ou remplie de boutons; voyez la Pl. XVI. Fig. 10. & XVII. Fig. 11. 12. où elles ſont repreſentées de grandeur naturelle et moienne. Elles reſtent dans le ſable, atachées à des cailloux, et étendent leurs membres à la ſuperficie pour ſaiſir la proye qui les touche. S'il eſt vrai que les têtes de pavots faſſent le deſeſpoir des peintres par la variété et l'éclat de leurs couleurs, on pourroit en dire autant des Anémones de Mer de cette grande eſpece; le beau blanc, le carmin, l'outremer, ſufiroient à peine. Quelques unes ont les membres d'une couleur douce et éteinte, tandis que la robe offre des couleurs riches et fort hautes.

La troiſieme eſpece ſemble s'écarter un peu plus de la ſeconde que celle ci de la premiere; elle eſt auſſi dans le ſable, ſon

corps

muſhroom, is terminated in its lower part by a baſis, which the animal fixes to the ſtones in the ſand, whilſt, by lengthening out its body, it affords means to the ſuperior part, where the limbs and the mouth are placed, of ſpreading out and opening themſelves at the ſurface of the ſand. See fig. 4. This ſpecies has ſome ſlight variety in point of ſhape, and ſtill more of colour. Some have their limbs of a bright white, or fine violet colour; others of an ivory white. Some are found of the ſame ſort of yellow with the inſide of melons. Some are greeniſh, or of a fine brown, with the middle white, which gives them a likeneſs to *Auriculas*. Others again have their limbs of a greyiſh tint, ſomewhat like the inſide of a broken piece of ſilver; or alternately mixed with black and white in the manner of the quills of a porcupine.

I ſhall now proceed to the particular experiments and diſcoveries I have made hitherto upon the three ſpecies.

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corps aſſez ſemblable pour la forme et pour la couleur au pédicule d'un champignon, eſt terminé à la partie inférieure par une baſe que l'animal atache aux cailloux dans le ſable, tandis que le corps en s'allongeant permet à la partie ſupérieure, où ſont les membres et la bouche de s'ouvrir à la ſuperficie, Fig. 4. Cette eſpece a quelques légères variétés de forme et encore plus de couleur; les unes ont les membres d'un beau blanc, ou d'un beau violet, d'autres couleur d'ivoire; il s'en trouve qui les ont couleur de chair de melon jaune; pluſieurs ſont verdâtres ou d'un beau brun avec un milieu blanc qui leur donne de la reſſemblance avec la fleur qu'on nomme oreille d'ours; d'autres ont les membres gris d'argent caſſé, ou mêlé alternativement de blanc et de noir, comme les piquans d'un porc épic.

Paſſons au détail des expériences et des découvertes que j'ai faites juſqu'ici ſur ces trois eſpeces. En juſtifiant ce que

ſpecies. In juſtifying what I laid down at firſt, they will convince us more and more what little progreſs has been made in the knowledge or hiſtory of *Reproductions*; how far the reſources and powers of nature may extend; the uncertainty of our conjectures both upon what really conſtitutes the animal, and the exact moment that it paſſes from life to death. What an extenſive field does the ſtudy of animal œconomy, that moſt beautiful and uſeful part of phyſic, offer ſtill to our inveſtigation! At the ſame time that it encreaſes our knowledge in that branch, which is looked upon as the baſis of the art of healing, it may help us to find out whether our ſea-flowers are deſtructive to crabs and other *cruſtaceous* fiſh, to muſcles, &c. and whether they themſelves might not prove a palatable and uſeful food. Theſe reſearches might alſo afford proofs that the ſtructure of thoſe animals which are looked upon as little deſerving of our attention, are ſtill, ſome

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que j'ai établi au commencement, elles pourront faire entrevoir combien nous ſommes peu avancés dans l'hiſtoire des reproductions; quelles peuvent être les reſſources et les forces de la nature; l'incertitude de nos conjectures ſur ce qui conſtitue l'animal, et ſur le moment précis où il paſſe de l'état de vie à l'état de mort. Quel vaſte champ nous offre encore à cultiver l'étude de l'œconomie animale! cet objet le plus beau et le plus utile que la phyſique nous préſente, eſt propre en même temps à augmenter la ſomme de nos connoiſſances dans cette partie qu'on regarde avec raiſon comme la baſe de l'art de guérir; à déterminer ſi ces animaux détruiſent les cancres et autres cruſtacés, les moules, &c. ou s'ils ne pourroient pas devenir eux mêmes un mêts recherché et délicat; à nous convaincre que la ſtructure des animaux qu'on juge peu dignes d'attention,

ſome by their complicate make, and others by their very ſimplicity, more incomprehenſible than larger and better-known animals. It may in ſhort keep up our admiration, by obſerving how the omnipotence of the Creator ſhines forth even in creatures, which, like theſe, ſeem produced to be trampled upon, or at beſt to be perceived by chance.

What firſt offered itſelf to my obſervations, is what diſtinguiſhes theſe animals from plants, I mean progreſſive motion, by the help of which they can ſhift their ſpot; the other determinate motions, by which they are enabled to lay hold of their prey; the means they make uſe of to defend themſelves; their deglutition, digeſtion, evacuations, and laſtly the propagation of their ſpecies, &c. What little I have had an opportunity to ſee of thoſe functions, appears to me ſufficient to place theſe creatures in the claſs of ſpontaneous animals, rather than in the dark indeterminate liſt of *zoophytes*.

In

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d'attention, offre dans les uns par ſa complication, dans les autres par ſa ſimplicité, quelque choſe de plus incompréhenſible que celle des animaux plus grands et plus connus; enfin à ſoutenir notre admiration en nous faiſant obſerver combien la grandeur de Dieu éclate juſque dans les créatures qui ſemblent comme celles-ci deſtinées à être foulées aux pieds, ou à n'être apercües que par hazard.

Ce que j'ai du d'abord obſerver, c'eſt ce qui diſtingue ces animaux des plantes, comme le mouvement progreſſif à l'aide duquel ils changent de place, les autres mouvemens déterminés par leſquels ils ſaiſiſſent la proye, les moyens qu'ils employent pour ſe défendre, la déglutition, la digeſtion, la déjection, la propagation de l'eſpece, &c. le peu que j'en ai vu m'a paru ſuffiſant pour leur aſſigner une place dans le regne animal, et pour les tirer du genre obſcur et indéterminé des zoophites.

Au

In May 1772, I clipped all the limbs of a purple Anemone of the firſt ſpecies. Soon after, theſe limbs began to bud out again. The 30th of July, they were clipped a ſecond time, and grew again in leſs than a month. Having cut them a third time, they had a third ſhooting out. The ſame experiment upon a green Anemone had the like ſucceſs. It ſeems theſe reproductions might extend as far, or be as often repeated as patience and curioſity would admit. Several experiments have convinced me that one ſingle limb of theſe Anemonies being cut off, retains a power to faſten itſelf to any ſmall body that is brought near it, either by its end, or by the ſide towards the end, but not by that part where the clipping was made. This induces me to think that the effect is produced by ſuction rather than by any glutinous matter, which one might ſuppoſe ouzes out at the pores. This limb, after being cut off, has alſo a power to ſtretch or contract itſelf alternately.

July

Au mois de Mai 1772, je coupai tous les membres à une anemone pourpre de la premiere eſpece. En peu de temps ces membres repouſſerent. Le 30 Juillet ils furent coupés de nouveau, et ſe reproduiſirent en moins d'un mois. Ayant été coupés une troiſieme fois ils ont encore repouſſé. Une anemone verte a donné le même réſultat. Il paroit que ces reproductions iroient auſſi loin qu'on auroit la curioſité ou la patience de les pouſſer. Pluſieurs expériences m'ont appris que juſqu'à un ſeul membre retranché de ces animaux, conſerve pendant pluſieurs jours la faculté de s'attacher aux corps qu'on lui preſente, ſoit par le bout, ou par le coté vers le bout, et non par la partie coupée; ce qui m'a porté à croire que cet effet pouvoit être produit par la ſuction plutôt que par la glu qu'on ſoupçonneroit exuder des pores. Ce membre coupé ſe contracte et ſe dilate auſſi alternativement.

July the 12th I cut one of theſe purple Anemonies through the body, rather nearer the baſis. This part remained adhering to the ſide of the veſſel in which it was; and for ſeveral days, made various motions. At laſt, it got looſe, and then faſtened in another place. The 27th it began again to move about, till the end of Auguſt, when it became as it were lifeleſs, grew very flabby, and had often an offenſive ſmell. I concluded it to be dead; but, as it did not loſe its ſhape, I reſolved to keep it, and to ſhift it every day into ſome freſh ſea-water. From time to time, I thought it had ſome ſort of motion, and in the beginning of November theſe motions became more perceptible. It ſhifted its poſition, when contrary to its natural ſtate. November the 28th, this *ſtump* climbed up to the top of the veſſel. I then began to perceive ſome new limbs growing out. January 13, 14,

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Le 12 Juillet je coupai par la moitié du corps et même plus proche de la baſe une anemone pourpre. Cette baſe reſta attachée aux parois du vaſe, et fit pendant pluſieurs jours différentes marches, après leſquelles elle ſe détacha, et s'attacha de nouveau. Le 27 elle recommenca ſes courſes juſqu'à la fin d'Aouſt, où elle ceſſa de donner ſenſiblement des ſignes de vie, devint d'une molleſſe extrême, et répandit même à pluſieurs repriſes une mauvaiſe odeur: je la jugeois morte; cependant comme elle ne ſe déformoit point, je pris la réſolution de la garder, et de lui procurer tous les jours de nouvelle eau de mer. Je crus appercevoir de loin à loin quelques mouvemens. Au commencement de Novembre, ſes mouvemens devinrent plus ſenſibles, et juſqu'à ſe retourner, quand elle étoit dans une poſition contraire à celle qui lui eſt naturelle. Le 28 Novembre elle grimpa au haut du vaſe, alors je crus appercevoir des membres renaiſſans. Les 13, 14, et 15 Janvier, elle a marché de nouveau

14, and 15, 1773, it again walked about; and on the 16th, ſeeing theſe growing new limbs, I offered them ſome bits of muſcles, which however were neither eat, nor even laid hold of. That ſame day, after ſeveral motions in various directions, it looſened its adheſion, and remained motionleſs and flabby, but without any bad ſmell, till the beginning of February, when it appeared adhering, but weakly, to the bottom of the vaſe. The 16th, after ſeveral motions, it climbed up to the top, where it remained till the 11th of March, and then looſened its hold. Theſe alternate ſtations and motions laſted till the 8th of April, without my ſeeing any plain and full re-production. However the animal continues to grow ſtronger and thicker. I own I was very wrong to throw away, a few days after the operation, the upper part that had been cut off. But I did not foreſee what would happen.

November

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nouveau. Le 16 appercevant ces apparences de membres je leur préſentai de petits morceaux de moule, qu'elle n'a ni mangés ni retenus. Ce meme jour, après pluſieurs mouvemens en divers ſens, elle s'eſt détachée et a reſté ainſi molle et immobile juſqu'au commencement de Fevrier, ſans pourtant porter de mauvaiſe odeur; enſuit eelle a paru foiblement attachée au fond du vaſe; elle a monté au haut le 16, après pluſieurs mouvemens, où elle a reſté juſqu'au 11 Mars, qu'elle ſe détacha. Ces alternatives et ces mouvemens ont duré juſqu'aujourd-hui 8 Avril, ſans que je voye paroitre de reproduction réelle; cependant elle continue de ſe fortifier et devient de plus en plus épaiſſe. Je jettai ſans doute fort mal à propos dès les premiers jours la partie retranchée; pouvois-je prévoir ce qui devoit arriver.

November the 9th, 1772, I clipped a brown Anemone through the body. The baſis, together with that part of the ſtump which was left to it, ſhrunk up, as figure 5. ſhews, and remained motionleſs where it was at firſt, till January 13, when it ſhifted its place. The 15th, I very diſtinctly perceived two rows of limbs growing out of the part where the ſection was made, fig. 6. and the animal moved along. The next day I offered it bits of muſcles, which it laid hold of and ate. Theſe growing limbs were, at firſt, of a ſullied white, they grew browner and browner every day, and are at preſent of the ſame colour with the coat of the animal. They are pretty near as large as they were before the operation; but I have not perceived, as yet, ſome of thoſe ſmall fine blue knobs, that are to be ſeen round the rim or upper *knurl* of the coat, as may be ſeen in figure 2. As to

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Le 9 Novembre je coupai par la moitié du corps une anemone brune; la baſe avec la partie du tronc qui lui reſtoit ſe retira ſur elle même, comme la repréſente la figure 5; elle reſta immobile au lieu où elle étoit attachée juſqu'au 13 Janvier qu'elle marcha. Le 15 j'apperceus très diſtinctement deux rangs de membres renaiſſans de la ſection même, figure 6, et l'animal marchoit. Le lendemain je lui préſentai des morceaux de moule; il les ſaiſit et les mangea. Dabord ces membres renaiſſans étoient d'un blanc ſale; mais peu à peu ils ont bruni et approchent maintenant de la couleur de la robe de l'animal. Il s'en faut très peu qu'ils n'ayent la même grandeur qu'ils avoient auparavant; je n'ai cependant point encore vû paroitre certains petits globes d'un beau bleu engagés dans le tour du bourlet ou bord ſupérieur de la robe, comme on peut voir par la figure 2: quant à la partie retranchée, repréſentée par

to the part cut off, ſeen in figure 7, which conſiſts of about half the body, and wherein the limbs and mouth are placed, I offered it, after the operation was performed, that brown part of a muſcle, by the help of which it moves along, and whence the beard ſpreads out. This bit, which is not eaſily digeſted by Sea-Anemonies, was directly ſnapped up by the limbs. They drew it to the mouth, which lengthened itſelf out to catch it, and ſwallowed it down. But, as the body was wanting to receive it, the bit came out at the oppoſite end, juſt as a man's head, being cut off, would let out, at the neck, the bit taken in at the mouth. I offered it a ſecond time, and the animal ſwallowed it again; but threw it up at the mouth the next day. I ſtill keep that part of the Anemone, which daily grows ſtronger and ſtronger, and which appears to me to ſuck in the bits of muſcles I offer it. The limbs lay hold of them, and the mouth takes them in, either whole or in part, and throws them up

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par la fig. 7. qui comprend environ la moitié du corps, où ſe trouvent les membres et la bouche, je lui préſentai peu après l'operation un morceau de moule où étoit ce membre brun qui lui ſert à ſe trainer, et où eſt attaché ſa filiere, morceau que les anemones de mer ont peine à digérer; les membres le ſaiſirent, l'approcherent de la bouche, la bouche même s'allongea pour le recevoir, et il fut avalé: mais comme il n'y avoit point de corps entier pour le recevoir, il ſortit par l'autre extrémité, comme une tête tranchée qui avaleroit pour rendre par le col. Je la lui preſentai de nouveau, elle l'avala une ſeconde fois et la rendit par la bouche le lendemain. Je garde encore cette partie qui ſe fortifie chaque jour et me paroit ſucer les morceaux de moule que je lui préſente; les membres les embraſſent, la bouche les reçoit en tout ou en partie, et me les rend fort altérés,

ſouvent

up a good deal altered; and pretty often theſe bits go through as they did the firſt time. Some perſons, who were eye-witneſſes of theſe particulars, were of opinion that, from the remains of organization and habit, this part of the animal ſtill endeavoured to gratify a natural want, though no longer ſubſiſting; but I am inclined to think that it ſtill exiſts. In my opinion the part is nouriſhed by means of ſuckers, of which I ſuſpect it to be full, both inwardly and outwardly. I am in expectation to have my conjecture confirmed, by experience, and by it to be enabled to convince other people. The microſcope ſeems to have already corroborated my notion upon this ſubject. When the limbs of theſe Anemonies, eſpecially thoſe of the *ſecond* ſpecies, are touched, the perſon's fingers are felt to adhere and ſtrongly to ſtick to the limbs. I have therefore let both theſe, and ſeveral other ſpecies of theſe animals, faſten ſeveral times upon the fingers of one of my hands, in order to ſee whether any glutinous matter ſhould remain upon them; but I never

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ſouvent ils paſſent d'outre en outre comme la premiere fois. Ceux qui en ont été témoins penſoient que, par un reſte d'organization et d'habitude, cette partie de l'animal cherchoit encore à ſatisfaire un beſoin qui n'exiſtoit plus; pour moi je ſuis porté à croire que ce beſoin exiſte, que la partie peut ſe nourrir par des ſucoirs dont je ſoupçonne qu'elle eſt remplie intérieurement et extérieurement, et j'attens de l'expérience ce qui me manque pour le prouver aux autres. Le microſcope m'a déja paru fortifier mon ſentiment; lorſqu'on touche les membres des anemones entr' autres de la ſeconde eſpece, on ſent ſes doigts fortement attachés à ces membres. Je les ai donc laiſſés s'attacher à pluſieurs repriſes et ſur pluſieurs anemones pour voir s'il ne reſteroit pas à mes doigts quelque ſorte de glu, et je n'y en ai jamais

never perceived any; and by applying the fingers of that hand to the other, I perceived no adheſion.

I have, for ſome days paſt, clipped ſeveral Anemonies of the firſt ſpecies, diametrically and perpendicularly to the baſis. They have ſtood the operation extremely well. Time will teach us what the reſult of theſe operations will be.

The 5th of February laſt, I placed a glaſs veſſel, expoſed to the morning froſt, with two Anemonies, the one brown the other purple, which ſtuck to the ſides of it towards the top. The ſurface of the water froze, and remained frozen the whole day: in the night-time, the thickneſs of the ice increaſed to ſuch a degree, that there remained but a little water not frozen in the bottom of the glaſs. The Anemonies were thus incloſed in a pretty ſtrong piece of ice, except the baſis, which remained adhering to the bottom. The glaſs was next day placed near the fire, in order to melt the ice by degrees, and when

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jamais apperçu : j'ai porté ces doigts ſur mon autre main, et il ne s'en eſt ſuivi aucune adhérence.

J'ai coupé depuis pluſieurs jours des anemones de la premiere eſpece diamétralement et perpendiculairement à leur baſe, elles ont ſoutenu l'operation fort bien ; le temps nous en devoilera les réſultats.

Le 5 Fevrier dernier au matin j'expoſai à la gelée un vaſe de verre contenant deux anemones, l'une brune, l'autre pourpre, attachées aux parois vers le haut du vaſe : la ſurface de l'eau ſe glaça, et reſta en cet état toute la journée ; pendant la nuit l'épaiſſeur de la glace augmenta tellement qu'il ne reſtoit qu'un peu d'eau dans le fond, de ſorte que de tous cotés excepté la baſe adhérente au verre, mes anemones étoient enfermées dans un fort glaçon ; le lendemain je mis le verre devant le feu pour faire fondre peu à peu la glace. Quand

when it began to float, I ſaw plainly the Anemonies were quite detached from the ſides, becauſe their body turned round looſe, with the ice, though their baſis had remained exactly in the ſame ſituation they were in before. Mr. *Reaumur's* thermometer being in my room, at two degrees above the freezing point, or o, theſe animals gave ſome ſign of life; and the 8th, the thermometer being at 3 ½ they opened; while the Anemonies, which were in other glaſſes, and had not been expoſed to a greater cold than that of one degree above froſt, did not open yet; and I generally obſerved they only opened at 5° of the thermometer. May it in conſequence be ſuppoſed that thoſe Anemonies, which had been frozen in the ice, were leſs affected with a cold at 3 ½ than the others?

March

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elle devint flotante, j'eus lieu d'apercevoir que les anemones avoient ceſſé d'être attachées aux parois du vaſe par leur baſe, parcequ'elles tournoient avec la glace, quoique leur baſe eut abſolument demeuré dans le même état où elles étoient auparavant. Le thermometre de Mr. de Reaumur étant dans mon cabinet à deux degrés de dilatation, ces animaux donnerent quelques ſignes de vie, et le huit le thermometre étant à 3 ½ s'ouvrirent tandis que celles des autres vaſes qui n'avoient point éprouvé de froid plus grand qu'un degré de dilatation ne s'ouvroient pas encore. Car j'ai remarqué qu'elles ne s'ouvrent, à parler généralement, que lorſque le thermometre parvient à cinq degrés. Celles qui ont été enfermeés dans la glace auroient-elles par comparaiſon trouvé la temperature de trois degrés et demi plus douce que ne la trouvoient les autres?

Le

March the 19th, I ſet upon the fire a glaſs veſſel, with the two animals which I had made uſe of for the foregoing experiments. A thermometer put into the ſame glaſs was at 8°; when it came to 15 and 17, the Anemonies opened themſelves a little; but, at 28 and 30, they appeared to be in pain; at 32 they cloſed themſelves up; at 37 one of the two fell off from the ſides; upon which I poured in ſome freſh water, and took the glaſs from the fire. Soon after they opened, and one of them produced two young ones the next day. In another experiment, the ſame Anemone which dropped off at 37 degrees, detached itſelf at 26, and cloſed up at 28. In a third experiment I ſhifted, at once, one of theſe Anemonies from 8° to 40°, and left it to remain in that heat for five minutes; it appeared to be in pain for ſeveral days after, its coat was even affected on one ſide, but, at

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Le 19 Mars je plaçai ſur un feu doux un vaſe de verre, contenant les deux anemones qui m'avoient ſervi aux expériences précédentes, avec un thermometre plongé dans le même vaſe; il étoit à 8 degrés de dilatation, lorſqu'il eſt parvenu à 15 et 17; les anemones ſe ſont un peu ouvertes; mais à 28 et 30, elles ont paru ſouffrir; à 32 elles ſe ſont fermées; à 37 une des deux s'eſt détachée des parois; j'ai alors promptement remis de nouvelle eau, et retiré le vaſe du feu, peu après elles ſe ſont ouvertes et une a donné deux petits le lendemain. Dans une autre expérience, la même anemone qui s'étoit détachée à 37 dégrés, s'eſt detachée à 26, et s'eſt fermée à 28. Dans une troiſieme expérience, j'ai fait paſſer ſubitement une de ces anemones de 8 degrés de dilatation à 40; je l'y ai laiſſée pendant cinq minutes; elle a paru ſouffrir pendant pluſieurs jours, ſa robe a même été entamée par un coté; mais enfin

at laſt it recovered its ſtrength, clung to the ſides, climbed up to the top of the glaſs; and all was well again. I have ſhifted another Anemone at once from 8° to 40°: but, inſtead of leaving it only five minutes, I went on increaſing the heat to 46°, in order to obſerve the effect. The coat appeared to me to ſwell out a little; at 50° it ſtill encreaſed, and the animal died in conſequence.

I put ſeveral Anemonies, ſome in, and ſome without, water, under the recipient of an air-pump; and got the quickſilver of the barometer incloſed in it, to come down within an inch of the level. The animals did not ſeem to perceive the effects of a *vacuum*, or an air ſo much rarefied. They remained open, or in whatever ſtate they happened to be when put in, and did not ſwell out. When the air was let in again, and they were not in the water, I perceived no ſhrinking at all in any of them.

Theſe

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elle a repris vigueur, s'eſt attachée, et a gagné le haut du vaſe; il n'y paroit plus. Du même point de 8 degrés j'ai fait paſſer ſubitement une autre anemone à 40, et au lieu de ne l'y laiſſer que cinq minutes, j'ai obſervé ce qui ſe paſſeroit en continuant de chauffer; à 46 degrés la robe m'a paru ſe bourſoufler un peu; à 50 degrés elle l'étoit encore plus, de ſorte que l'animal en eſt mort.

J'ai mis dans l'eau, et ſans eau, ſous le récipient d'une machine pneumatique pluſieurs anemones, j'ai abaiſſé le mercure du barometre qui y étoit renfermé, juſqu'à un pouce de ſon niveau. Elles n'ont pas paru s'appercevoir de l'effet du vuide, ou d'un air ſi fort raréfié: elles ont reſté ouvertes, ou comme elles étoient, ne ſe ſont point gonflées, et lorſque j'ai rendu l'air au récipient, et qu'elles n'étoient point dans l'eau je n'ai apperçu en elles aucun affaiſſement.

Il

Theſe animals can live a whole twelvemonth, and perhaps much longer, without any other food but what they chance to find diſſeminated in the ſea-water. They do not want many motions to procure their food, beſides ſtretching out their limbs, to receive ſuch as comes within their reach; and they remain ſurrounded with muſcles, &c. without laying hold of any of them. I have given Anemonies ſome of theſe muſcles alive, but with their ſhells cloſed, and about ſix lines in length. They were ſwallowed in that ſtate; and 40, 50, and 60 hours after, the ſhells were thrown up at the mouth empty and perfectly cleared, even from the ſmall tendons which connect the fiſh to its ſhells. The Anemonies ſwallow and digeſt ſmall fiſh, and bits of larger fiſh, or of raw meat, when offered to them. When they cannot digeſt ſome of the food, they throw it up at the mouth, either whole or partly diſſolved into

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Il paroit que ces animaux peuvent vivre pendant une année, et peut-être beaucoup plus, ſans autre nouriture que celle qu'ils trouvent diſſéminée dans l'eau de la mer; qu'ils ne font pas beaucoup d'autres mouvemens pour s'en procurer d'autre que celui d'étendre leurs membres pour recevoir celle qui vient d'elle même y toucher: car ils reſtent environnés de moules, &c. ſans s'en emparer. Je leur en ai donné de 6 lignes de longueur vivantes et fermées, les anemones les ont avalées en cet état, et 40, 50, ou 60 heures après, elles m'ont rendu par la bouche les deux coquilles vuides et parfaitement nettoyées, même des petits tendons qui attachent le poiſſon aux deux coquilles. Elles avalent et digerent des petits poiſſons, et des morceaux de poiſſon ou de viande fraiche que je leur préſente. Lorſqu'elles ne peuvent digérer quelque choſe, elles le ren-

into a viſcous liquor, which may in ſome meaſure be looked upon as their excrements. They void by the ſame way, and in the ſame manner, various parts of a white and brown ſubſtance, and ſmall bodies are thrown out at the extremity of the limbs, and more ſenſibly in the ſecond ſpecies. There alſo ouzes out of their body a ſort of excrementitious matter, which by coagulating produces round them a ſort of girdle.

Theſe animals are known to be *viviparous*. Several of them have brought forth, even in my hand, 8, 10, and 12 young ones. Some, though almoſt imperceptible, as in fig. 8, have nevertheleſs the power of clinging, and are endowed with two rows of limbs, which they open immediately upon their birth, in order to catch their prey, which they ſwallow afterwards. I have kept ſome for ten months. They have appeared to me not to have increaſed in their bulk, more than twice

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dent par la bouche ou en entier ou réduit en partie en une eſpece de glaire qu'on peut regarder comme leurs excrémens, quoiqu'on leur voie rendre auſſi par la même voye différentes parties de matiere blanche ou brune, et de petits corps par le bout de leurs membres, ce qui eſt plus ſenſible dans la ſeconde eſpece. Il ſort auſi de leur corps une ſorte d'humeur excrémentitielle, qui forme tout autour en ſe coagulant comme une eſpece de ceinture.

Ou ſait que ces animaux ſont vivipares; pluſieurs m'ont donné, même dans ma main 8, 10 et 12 petits, les uns preſqu'imperceptibles, figure 8, n'en ont pas moins la faculté de s'attacher, et ont un ou deux rangs de membres qu'ils ouvrent auſſitôt après leur naiſſance pour ſaiſir la proye qu'ils avalent enſuite; j'en ai gardé pendant dix mois; ils ne m'ont paru augmenter en groſſeur que du double de leur diamètre. Il eſt

twice the diameter of their ſize. Indeed I fed them ſparingly; the others are grown as big as half a green pea. Fig. 9.

The Sea-Anemonies have a progreſſive motion; which, though ſlow, is performed in every direction, with a degree of facility, and effected by means of muſcles, which croſs one another at right angles. I cannot as yet diſplay the mechaniſm of theſe muſcles, becauſe the laſt mutilations I was obliged to make diſcompoſe the conjectures that ſome men of learning have publiſhed upon this ſubject; and I have not ſufficiently fixed my thoughts in conſequence of my obſervations.

Theſe animals, being put into freſh water, die very ſoon after, and whatever their colour be when they are put in, they ſoon turn pale, and remain pretty near in the ſame poſition that they were in. Soon after, their coat becomes ſo flabby, that it can very

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vrai que je leur ai donné peu de nouriture; les autres ſont gros comme la moitié d'un pois verd, Fig. 9.

Les anemones de mer ont un mouvement progreſſif, à la verité aſſez lent, mais qui s'exécute pourtant en tous ſens avec une ſorte de facilité. Il dépend de muſcles qui ſe croiſent à angles droits; mais je ne puis en développer ici le méchaniſme, parceque les dernieres mutilations que j'ai faites déconcertent les conjectures que quelques ſavans avoient publiées ſur ce point, et qu'en conſéquence je n'ai pu encore fixer mes idées à ce ſujet.

J'ai mis des anemones de mer dans de l'eau donce, elles y ont péri preſqu'auſſitôt, on les voit palir un peu, de quelque couleur qu'elles ſoient et demeurer à peu près dans la même poſition qu'elles étoient au moment qu'on les y a plongées. Peu de temps après, leur robe devient ſi facile à entamer, qu'un pein-

very eaſily be ſtripped off in ſhreds. For this reaſon, when I boiled them, I always made uſe of ſea-water.

I have not been able hitherto to perceive any eyes in theſe creatures; and yet they ſeem to be affected by the light. I have had opportunities to ſatisfy myſelf on this ſubject at every hour of the night. For by holding up a lighted candle over glaſſes, which contained a great number of them, but at ſuch a diſtance as not to communicate any heat, I could ſee them, either by degrees, or even ſuddenly, cloſe up, and not ſpread out again 'till a while after the light was removed. The ſame effect was produced upon repeated trials. This, however, admits of ſome exceptions, viz. when they had fed plentifully, they were ſlower in ſhutting themſelves up, and even ſometimes did not cloſe up at all. The brighter the light let in upon them

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pinceau l'enleve aiſément par lambeaux; c'eſt pourquoi quand j'en ai fait cuire, j'ai employé de l'eau de mer.

Juſqu'ici les anemones ne m'ont point paru avoir d'yeux, elles ſont pourtant affectées par la lumiere. J'ai eu occaſion de le remarquer à toute heure de la nuit; car en tenant au deſſus des vaſes, qui en contenoient un grand nombre, une bougie allumée de laquelle elles ne pouvoient reſſentir aucune chaleur, je les voyois peu à peu quelquefois même ſubitement ſe fermer, et elles ne s'ouvroient de nouveau que lorſque je faiſois diſparoitre la lumiere pour quelque temps, après lequel ſi je la leur préſentois, elles ſe fermoient comme la premiere fois. Ceci a quelques exceptions qui ne détruiſent point la loi générale; par exemple, lorſqu'elles ont beaucoup mangé elles ſe ferment moins vite et même point du tout. Plus la lumiere eſt vive en proportion de celle dont elles jouiſſoient au

them is, in proportion to the degree of that which they were in before, the more affected do they appear to be by it. Is then the texture of the body of thoſe creatures ſuch, as not, indeed, to receive the impreſſions of objects, with the ſame degree of perfection as our eyes do; but, to afford a general organ affected by light, as the bodies of other animals are by feeling?

Might not theſe animals become a freſh ſupply for our tables; and thereby make amends for the conſumption of muſcles, on which they feed? In order to try the experiment, I gave ſome Anemonies, boiled in plain water, to a cat, who devoured readily twenty of them, ſeveral of which were of the third ſpecies, without any bad effect, and ſeemed wiſhful to have more. This induced me to make a further trial, and to get ſome of them colloped as we do oyſters in

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auparavant, plus elles y paroiſſent ſenſibles. Le corps de ces animaux ſeroit-il donc, je ne dis pas un organe capable de recevoir l'impreſſion des images, avec les conditions néceſſaires à une ſenſation auſſi parfaite que la procure notre oeuil, mais une eſpece d'organe général ſenſible au contact de la lumiere comme le toucher des autres animaux l'eſt à celui des autres corps?

Ces animaux ne pourroient-ils point devenir une nouvelle reſſource pour nos tables, et réparer ainſi la perte des moules dont ils ſe nourriſſent? pour m'en aſſurer, j'en fis donner de bouillis, ſans aucun aſſaiſonnement, à un chat, qui en mangea avec avidité vingt, dont pluſieurs étoient de la troiſieme eſpece; il n'en parut reſſentir aucune incommodité, on jugea même à ſon air qu'il en auroit mangé d'avantage. Cela me fit déſirer d'avoir ſur cet objet eſſentiel une plus grande certitude, j'en fis préparer comme on fait les huitres dans ces grands peignes, qu'on nomme coquilles de pélerin, et les ai mangés

in ſhells, I ſwallowed them up myſelf. Perhaps they may prove ſtill more palatable, dreſſed in ſome other manner. However, they may be eat ſafely.

The ſecond ſpecies of Sea-Anemonies keeps itſelf hid more than the firſt; it is not to be come at but in neap-tides, when the ſea recedes fartheſt, and cannot be ſo eaſily obſerved. With great difficulty are theſe Anemonies looſened from the ſtones they adhere to; part of the baſis is often left behind, and they are not eaſily preſerved at home. Seeing however ſome of the individuals that voided muſcle-ſhells whole, and ſtill joined together, but empty, I found out the way to feed them. Crab-ſhells, about the ſize of a hen's egg, are likewiſe diſcharged whole; and upon offering them ſome live-ones, I found that they ſwallowed them down, and voided the remains ſucked dry in about twenty hours. I cut open ſome of this ſecond ſpecies, which I could not looſen from the ſtones, and among them I found

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mangés ; peutêtre une autre préparation leur conviendroit-elle mieux, toujours eſt-il certain qu'on en peut manger.

La ſeconde eſpece d'anemones de mer plus cachée que la premiere, et qu'on ne trouve que dans les grandes marées, où la mer ſe retire plus loin, eſt auſſi plus difficile à obſerver ; on a peine à la détacher des cailloux ſans y laiſſer une partie de ſa baſe, et à la conſerver chez ſoi. C'eſt en obſervant quelques individus de cette eſpece et en appercevant qu'ils rendoient des coquilles de moules entieres, jointes enſemble et vuides, que j'ai appris de quelle maniere je devois nourrir mes anemones. Je leur ai vu même rendre des cancres entiers grands comme la circonference d'un oeuf de poule, je leur en préſentai de vivans qu'elles avalerent et qu'elles m'ont rendus morts, et ſucés environ vingt heures après. J'en ai ouvert que je ne pouvois détacher des cailloux, parmi leſquelles j'en

trouvai

one that had ſwallowed an Anemone of the *third* ſpecies; but this had received no harm. For, having put it into ſome ſea-water, it opened and ſpread as uſual. I have offered them ſeveral of the ſame ſpecies, which they ſwallowed down; but threw up again alive within eight, ten, or twelve hours, or even later. Is then a live Anemone an undigeſtable body for another?

On the 21ſt of June, 1772, having *nicked* the inſtant, that an individual of the third ſpecies was ſtretching out, as expreſſed in Fig. 13, I ſnipped off at once with ſharp ſciſſars, the whole upper part where the limbs and mouth are placed. It was with great ſatisfaction that, eight days after, I perceived new limbs growing out, as in Fig. 15. The third of July, the animal began to eat ſome bits of muſcles; and towards the middle of the month, the upper part was ſo completely formed, that it might eaſily

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trouvai une qui avoit avalé une anemone de la troiſieme eſpece qui n'en avoit ſouffert aucune altération: car l'ayant miſe dans l'eau de mer, elle s'ouvrit comme à l'ordinaire. Je leur en ai préſenté pluſieurs, elles les ont rejettées en vie au bout de huit dix ou douze heures, et même plus tard. Une anemone en vie ſeroit-elle donc un corps indigeſte pour une autre anemone?

Le 21 Juin ayant ſaiſi l'inſtant où une des anemones de la troiſieme eſpece étoit allongée, comme dans la figure 13, j'en retranchai ſubitement, avec de bons ciſeaux, toute la partie ſupérieure où ſont les membres et la bouche. Avec quelle ſatisfaction n'ai-je pas vu au bout de huit jours de nouveaux membres renaiſſans figure 15. Le trois Juillet l'anemone commença à manger des morceaux de moule, et vers la moitié du même mois, la partie ſupérieure étoit ſi bien reproduite,

eaſily have been miſtaken for one of its unclipped neighbours, had there been many in the glaſs. It is neither the row of the centrical or inner limbs, nor the moſt outward, which firſt bud out, but the intermediate ones. The part, which had been clipped off, gave ſigns of ſenſibility to the 17th of July, contracting and dilating itſelf, in the ſame manner as a whole Anemone; but it was much ſmaller than before the operation. This experiment has been repeated by clipping, on the 11th of July, the whole upper part and one third of the body of another Anemone. New limbs began to ſhoot out the 21ſt. There were two rows of them the 25th; and on the 3d of Auguſt, four very diſtinct and well ſhaped, which caught and kept faſt the food that was offered the animal. The mouth itſelf was ſufficiently well formed to take in ſeveral times bits of muſcles.

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qu'on auroit aiſément confondu l'anemone avec ſes voiſines, ſi elles euſſent été en grande quantité dans le même vaſe. Ce ne ſont ni les rangs des membres les plus voiſins du centre, ni ceux qui en ſont les plus eloignés qui paroiſſent les premiers dans la reproduction, mais les intermédiaires. La partie qui avoit eté retranchée a donné juſqu'au dixſept Juillet des marques de ſenſibilité, ſe contractant et ſe dilatant de la même maniere que le fait l'anemone: mais elle étoit beaucoup plus petite qu'avant d'être retranchée. Cette expérience ſinguliere a été repétée en coupant le onze Juillet toute la partie ſupérieure et un tiers de la longueur du corps d'une autre anemone. Il reparut de nouveaux membres le vingt-un; le vingt-cinq elle en avoit deux rangs, le trois Aouſt quatre bien formés et qui retenoient les corps qu'on lui préſentoit, lorſqu'ils étoient propres à la nourriture: la bouche même commençoit à être aſſez bien formée pour que l'animal ait mangé pluſieurs fois de

muſcles. On the 11th, I perceived in the limbs the faint alternate marks of ivory white and black, and ſoon after, there ſcarcely appeared any ſign of the operation having been performed.

Being induced to try further experiments, on the 7th of Auguſt, I clipped an Anemone acroſs the body. Like the others, it moved or wriggled a little at firſt: but, I did not perceive any new limbs growing till towards the end of the month. During that time, it continued in ſuch a ſtate as gave but little hopes of its doing well again. Two rows of limbs appeared at laſt, and the inſect recovered its ſtrength. There was on the 9th of September a third row of limbs, and the mouth appeared to be ſhaped out and formed; yet the Anemone neither ate nor kept the bits of muſcles I gave it. A fourth row was growing out on the 19th, which gathered ſtrength by degrees; ſo that on the

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des morceaux de moule. Le onze j'aperçus dans les membres les petites alternatives de blanc d'ivoire, et de noir; et peu après il étoit difficile de s'appercevoir qu'elle eut ſouffert quelque altération.

Tenté de pouſſer la choſe plus loin, le ſept Aouſt je coupai par la moitié du corps une anemone. Elle ſe donna d'abord comme les autres quelques mouvemens, mais ce ne fut que vers la fin du mois que j'obſervai de nouveaux membres; pendant cet intervale elle fut dans un état qui laiſſoit peu d'eſpoir, enfin deux rangs de membres parurent et l'animal reprit vigueur. Le neuf Septembre il y avoit un troiſième rang de membres, et la bouche paroiſſoit formée; l'anemone cependant ne mangeoit ni ne retenoit les morceaux de moule qui lui étoient préſentés: le dixneuf on aperçut un quatrieme rang de membres, qui peu à peu ſe fortifia, de maniere que le trois

the 3d of October it began to eat, and in a ſhort time became a compleat animal. On September the 22d, the upper part appeared to me to be withering away. But we ſhall ſoon ſee how much I have reaſon to think I was miſtaken.

I cut another Anemone, of the ſame ſpecies, acroſs the middle of the body, in ſuch a manner as that the two parts were only left hanging together by one fourth part of the diameter. My deſign was, to try whether nature would produce limbs on the edge of the lower half-part, in the ſame manner as when the body is cut quite aſunder; or, whether the wound, though very deep, would heal up again. Nature was not wanting to itſelf; for, notwithſtanding the largeneſs of the inciſion, the two ſevered parts were joined up together, and in a few days the wound was healed

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Octobre l'anemone mangea et devint en peu de temps un animal complet. Le partie ſupérieure me parut périr le vingt deux Septembre; mais on verra combien j'ai lieu de croire que je me ſuis trompé.

J'ai encore coupé par la moitié du corps une anemone de cette eſpece, de maniere que les deux parties ne tenoient plus l'une à l'autre que par un quart du diametre du corps de l'animal. J'avois envie de voir ſi la Nature ne feroit pas naitre des membres au coté coupé de la partie inférieure, comme lorſque la ſupérieure en eſt totalement ſéparée, ou ſi la playe quoique fort grande ſe conſolideroit. La Nature ne s'eſt point trompée; malgré la grandeur de l'inciſion les deux parties ſe ſont rapprochées, et au bout de quelques jours la playe

healed up. The animal did not even ſeem to have ſuffered ſo much as one might have been apt to expect.

I have been witneſs to a fact remarkable enough to be inſerted here. Having ſliced a bit of fiſh, I offered one end of it to an Anemone, whoſe limbs are of the melon-yellow colour, and the other end to a grey Anemone, whoſe ſuperior part had been re-produced after it had been cut off. The two inſects, which were adhering pretty near one another at the bottom of the glaſs, directly ſeized their prey with their arms; but the yellow one happened to lay hold of the larger ſhare of the ſlice. Each ſwallowed on, by the reſpective ends, till at laſt each other's mouth came within contact. The grey one ſeemed, at firſt, to get the better; but the other ſoon recovered her ſhare, loſt it again, and again recovered it. Theſe alternate victories laſted about three hours; and

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playe étoit conſolidée. L'animal n'en a pas même paru ſouffrir autant qu'on l'auroit imaginé.

Jai été témoin d'un fait aſſez ſingulier pour qu'il trouve place ici. Ayant coupé un morceau de poiſſon en forme d'aiguillette, je le préſentai par un bout à une anemone, dont les membres ſont couleur de chair de melon, et de l'autre bout à une anemone griſe, dont la partie ſupérieure s'étoit reproduite après avoir été coupée. Mes deux anemones attachées au fond du vaſe aſſez près l'une de l'autre ſaiſirent la proye avec leurs membres; mais l'anemone jaune eut lieu d'en embraſſer une plus grande partie. L'une et l'autre avalerent le bout qu'elles tenoient et continuerent d'avaler juſqu'à ſe trouver bouche à bouche. La griſe me parut avoir l'avantage; peu après il lui fut diſputé, elle le recouvra, puis le laiſſa échaper, enſuite le reprit. Ces alternatives ayant duré pendant trois heures, je

vis

and there was a time, during which the yellow Anemone was near being worſted; till at laſt, the grey one loſing hold of her end of the ſlice, the other carried off the prize. Yet, as ſhe ſucked in but ſlowly, the grey one ventured with her mouth upon a laſt tug at her end, ſtill in ſight, which ſhe had ſlipped; but this freſh effort proved fruitleſs: the yellow champion gave a laſt pull, and ſwallowed down the whole. During this whole ſtrife, the two parties did not ſeem to be animated by any other paſſion than that of ſnatching the ſlice of fiſh from one another; and though the two animals continued afterwards to remain neighbours, they lived very quiet and peaceable together.

Theſe animals are ſometimes very voracious. Could it be believed that the ſame creature, that can continue in pretty good plight for a whole twelvemonth, and perhaps longer, without taking in any other food, beſides what may

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vis un moment où les affaires de la jaune paroiſſoient aller aſſez mal, lorſqu'enfin la victoire ſe déclara pour elle: la griſe lacha le bout. Comme la jaune n'avaloit pas très vite, la griſe eut le temps de revenir à la charge; elle ſaiſit de nouveau par la bouche le bout, qu'elle venoit d'abandonner, mais ce fut inutilement; la jaune fit un nouvel effort, et le morceau fut avalé. Il ne parut dans ce combat ſingulier d'autres efforts que ceux qui tendoient à s'entre arracher la proye, et depuis les deux anemones toujours voiſines ont vécu auſſi paiſiblement qu'auparavant.

Cependant les anemones ſont quelquefois très avides. Croiroit-on que cet animal, qui peut vivre en aſſez bon état pendant une année, et peut-être plus, ſans prendre d'autre nourriture que celle qu'il trouve diſſéminée dans l'eau

may be diſſeminated in the ſea water, and does not ſeem very active to lay hold of it's prey, but rather waits patiently till chance throws it within reach of its limbs; that this animal ſhould ſtill be ſo greedy as to gulp down two whole muſcles, I gave one of them by piece-meal, and burſt the next day with indigeſtion, when it has a power of throwing up ſo eaſily what it has ſwallowed down? This was the caſe with an Anemone of the third ſpecies, and of a middle ſize, which had been fiſhed lately.

On the 8th of October, the ſame Sea-Anemonies (of which I had clipped out the upper part with the mouth and limbs, inſtead of which new ones were re-produced, perfect enough to enable them to eat) were divided a ſecond time, and again renewed, ſo as not to be diſtinguiſhed from thoſe which had undergone no operation. More than

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l'eau de la mer, et qui ne paroit pas ſe donner de grands mouvemens pour attraper la proye qui l'environne, qui au contraire attend patiemment que le hazard la faſſe tomber entre ſes membres, ſoit aſſez glouton pour avaler en deux heures la valeur de deux groſſes moules, qu'on lui préſente par morceaux et crever d'indigeſtion le lendemain; tandis qu'il peut rendre aiſément ce qu'il avale? C'eſt ce qui eſt arrivé à une anemone de la troiſieme eſpece et de moyenne groſſeur, qui étoit récemment pêchée.

Le huit Octobre les mêmes anemones de mer, dont j'avois retranché la moitié du corps où ſe trouvent les membres et la bouche, et auxquelles toutes ces parties s'étoient reproduites, juſqu'à leur permettre de manger, ont été coupées de nouveau et ont repouſſé pour la ſeconde fois ſi bien, qu'il n'eſt plus poſſible de les diſtinguer de celles qui n'ont ſouffert aucune alteration

than that, having taken particular care to feed one of theſe halves clipped for the ſecond time, I ſaw it grow ſtronger and ſtronger every day, and perform with equal facility the ſame functions as any other compleat original Anemone. The only difference or exception is, that its baſis is not yet perfect enough, as to enable the animal to adhere or fix itſelf to the glaſs; I make no doubt however, but what this new Anemone will, in a ſhort time, acquire the only powers yet wanting, to render it a perfect one; ſee fig. 16 and 17. Might not one aſk, upon all theſe facts, what is become of the original animal? is it that which continued adhering by its baſis to the glaſs? or is it the upper half? Are there animals among which an *individual* is not a *ſimple* being?

There oozes habitually out of theſe inſects an excrementitious humour, which thickens round their body. At periodical times it iſſues out in a greater quantity, and aſſumes then a *vermi-*

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teration. Il y a plus, ayant pris ſoin de nourrir une des parties retranchées, par la ſeconde coupe, je l'ai vue ſe fortifier de jour en jour; et faire avec la même aiſance toutes les fonctions de l'anemone parfaite, excepté que la baſe n'eſt point encore en état de s'attacher, mais elle s'élargit et je ne doute point par les progrès que je lui vois faire que la nouvelle anemone n'aquiere dans peu le ſeul point qui lui manque, pour arriver à l'état parfait, voyez les figures 16 et 17. Ne pourroit-on pas demander maintenant qu'eſt devenu le premier animal? eſt-ce celui qui a été retranché, eſt-ce l'autre? y auroit-il donc des animaux où un invidu ne ſeroit point un être ſimple?

Il ſort habituellement du corps de ces animaux une humeur excrémentitielle qui s'épaiſſit; mais elle en ſort auſſi périodiquement en plus grande abondance, et y prend alors une forme

cular appearance. During that period, the Anemone ſeems to be ſickly; but afterwards recovers its ſtrength.

It is upon this ſpecies that I had particular opportunities of obſerving, that when, by cutting, there remains but the baſis, and a ſhort ſtump of the body; the animal is equally, if not more, affected by the light, than before the operation. The ſame effect is produced upon the part where the mouth and limbs are.

I may perhaps be taxed with cruelty, with regard to theſe inſects. But, I believe, I ſhall ſoon be abſolved of the charge, when it is conſidered what favourable effects my experiments have had upon the Anemonies which have been the ſubject of them; as I have not only extended their exiſtence, but likewiſe renewed their youth; and this ſure is no ſmall advantage.

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forme vermiculée; pendant ce temps l'anemone paroit malade, mais après elle reprend une nouvelle vigueur.

C'eſt particulierement ſur cette eſpece que j'ai eu lieu d'apercevoir que lors même qu'il ne reſte à ces animaux que la baſe et un tronc aſſez court, ils ſont autant et plus affectés par la lumiere, qu'avant d'être mutilés. Il en eſt de même de la partie, où ſont les membres et la bouche.

On pourroit être tenté de m'accuſer de cruauté envers ces animaux, mais ſi l'on fait attention aux effets qu'ont produit mes expériences, ou ſoupçonnera aiſément que pluſieurs ne peuvent que ſe féliciter d'en avoir été l'objet; car non ſeulement j'ai étendu leur être, mais probablement je les ai rajeunis, ce qui n'eſt pas un petit avantage.

What I have mentioned of the effects of freſh water upon the firſt ſpecies of Sea-Anemonies, of the colour of the *re-produced* limbs, of their way of ſwallowing whole live muſcles, &c. muſt be likewiſe underſtood of, and applied to this third ſpecies; and, in many reſpects, to the ſecond.

I am ſenſible that many things are ſtill wanting in this paper; but what I have wrote down, is very near all that I can infer from my firſt obſervations and experiments, without expoſing myſelf to the neceſſity of falling back in regard to eſſential things.

POST-

Ce que j'ai dit des effets de l'eau douce ſur la premiere eſpece, de la couleur des membres reproduits, de la maniere d'avaler les moules, &c. doit s'appliquer à celle-ci, et en grande partie à la ſeconde.

Je laiſſe ſans doute beaucoup à deſirer ici, mais ce qui y eſt expoſé eſt preſque tout ce que je puis conclure de mes premieres obſervations et de mes premieres expériences, ſans riſquer de me mettre dans la neceſſité de revenir ſur mes pas pour des choſes eſſentielles.

POST

POSTSCRIPT.

Havre de Grace, April 28, 1773.

JUST as I concluded this eſſay, I found out a *fourth* ſpecies of Sea-Anemonies, of the ſize of the ſecond, and of an elegant form, having the appearance of a cluſter of white or fleſh-coloured feathers. Theſe Anemonies are found in oyſter-beds, &c. I have obſerved that there grows or comes out of their body and mouth a ſort of threads about the ſize of a horſe-hair, which being examined with a ſolar microſcope of five inches diameter, appear as if made up of a prodigious number of veſſels, wherein a liquor is ſeen to circulate. The largeſt

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POST SCRIPTUM.

Le Havre, 8 Avril, 1773.

LORSQUE je terminois ce mémoire, j'ai découvert une quatrieme eſpece d'anemones de mer, groſſe comme la ſeconde, et d'une forme élégante, repréſentant comme un aſſemblage de panaches blancs ou couleur de chair. On la trouve dans les huitrieres, &c. J'ai remarqué qu'il ſort de ſon corps et de ſa bouche certains cordons de même couleur déliés comme un crin, leſquels vus au microſcope ſolaire ſous un diametre de 5 pouces, paroiſſent formés d'une quantité innombrable de vaiſſeaux où circule une liqueur. Les plus gros

 s'uniſſent

largeſt of theſe unite together, much in the ſame manner as the optic nerves do in man. Such an organization is doubtleſs intended for moſt important purpoſes. Some young ones of this ſpecies, which ſtill adhered to one another by a ſtring of communication, ſhut themſelves up in the ſame inſtant, when this ſtring was touched in the middle. As I could not directly contrive a total ſection of this large ſpecies, I tried it upon the young ones; and theſe ſhooted out again after the operation; (and ſo have the old ones done ſince). I have met with a ſort of monſter among theſe Anemonies, *viz.* one which ſeemed to incloſe or contain three others, two of which were united at their baſis, and the third lay, as it were, concealed in the folds.

Nature has reſources little known to us; it ſeems ſometimes to vary its operations, with an intent, as it were, the more to ſtimulate our curioſity

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s'uniſſent en ſe rencontrant à peu près comme les nerfs obliques dans l'homme; une telle organiſation eſt ſans doute deſtinée à des uſages précieux. Des petits de cette eſpece, qui étoient encore adhérens l'un à l'autre par un cordon de communication, ſe fermoient en même tems lorſque je touchois ce cordon par le milieu. Ne pouvant d'abord ſoumettre cette grande eſpece à une ſection totale, je l'ai eſſayée ſur de jeunes individus, ils ont repouſſé; (les grands repouſſent de même). J'ai eu dans cette eſpece un monſtre, c'eſt-à-dire une anemone qui paroiſſoit en contenir trois, dont deux oppoſées par leur baſe et une troiſieme cachée dans les plis.

La Nature a des reſſources qui nous ſont peu connues, et ſemble quelquefois varier ſes procédés à deſſein de piquer de plus en plus notre curioſité, et peut-être de dévoiler ſes ſecrets, à ceux

rioſity, and perhaps to diſcloſe her ſecrets to thoſe who are endowed with a degree of ſagacity or patience, ſufficient to follow and inveſtigate the effects offered to our obſervations. Amidſt the great number of Anemonies of the third ſpecies which I have clipped acroſs the body, there have happened to be two, whoſe lower part has in the uſual way ſhot forth new limbs: but the *upper half*, where the limbs and mouth were, inſtead of healing up into a new *baſis*, has produced both another mouth and limbs. Hence an animal was formed, which caught its prey, and fed at both ends in the ſame time.

The Sea-Anemonies of the three firſt ſpecies mentioned before, and perhaps thoſe likewiſe of the fourth, feed upon thoſe floating tranſparent animals of a white glaſſy, or of a blue or purpliſh hue, called *wandering nettles*, or *ſea-jellies*. An Anemone of a middle ſize, of the firſt and third ſpecies,

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ceux qui ont aſſez de ſagacité, ou même de patience pour ſuivre les effets qu'elle nous préſente. Dans le nombre conſiderable d'anemones de la troiſieme eſpece que j'ai coupées par la moitié du corps, il s'en eſt trouvé deux, dont la moitié inférieure a repouſſé de nouveaux membres comme à l'ordinaire: mais la moitié ſupérieure où étoient les membres et la bouche, au lieu de ſe conſolider et de former une baſe à la ſection, y a reproduit des membres et une bouche, de ſorte qu'il en a réſulté des animaux, qui ſaiſiſſent la proye, et mangent par les deux bouts en même temps.

Les anemones de mer des trois premieres eſpeces, et peut-être ceux de la quatrieme, ſe nourriſſent de ces animaux flotans et tranſparens, le plus ſouvent de couleur de verre blanc, ou bleux, ou violets, connus ſous les noms d'*Orties errantes*, *de gelées de mer*, &c. Une anemone de la premiere et de la troiſieme eſpece comme

ſpecies, ſuch as that repreſented by Figures 1, and 13, ſwallows one of theſe animals of the ſize of half an orange. All theſe four ſpecies are good to eat.

Particular EXPLANATION of ſome of the FIGURES.

Plate XVI. Fig. 14. ſhews the Anemone of the third ſpecies, when ſhrunk up. One ſees round it a ring of ſand and broken pieces of ſhells ſticking together by means of the excrementitious humour habitually oozing out of the body of the animal, or out of the little granulated knobs, with which it is covered towards the upper part. This ring is alſo to be ſeen in the ſame Anemone, when lengthened out, as expreſſed in Fig. 13.

Fig.

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comme celles des figures 1 et 13, avalent un de ces animaux gros comme la moitié d'une moyenne orange. Toutes ces 4 eſpeces d'anemones de mer ſont très bonnes à manger.

EXPLICATION de quelques unes des FIGURES.

Planche XVI. On voit à la figure 14 une anemone de la troiſieme eſpece, dans ſon état de contraction. L'eſpece de cordon, qui paroit tout autour, eſt formé de ſable et de fragmens de coquillages joints enſemble par cette humeur excrémentitielle, qui ſort habituellement du corps de l'animal, ou des petits mammelons dont la partie ſupérieure eſt revêtue. Cet anneau s'apperçoit auſſi dans la même anemone, lorſqu'elle s'allonge, fig. 13.

Dans

Fig. 10. ſhews a Sea-Anemone, of the ſecond ſpecies, concealed under the ſand, and covered over in different places with broken ſhells and gravel, with which the animal forms a coat of mail to ſecure itſelf under, but out of which it can ſlip in an inſtant. The figure ſhews it when it ſpurts out water at its mouth, and at the end of its limbs.

Plate XVII. Fig. 11. ſhews the ſame Anemone open. The mouth is in the center of the upper part: it is not always ſhaped in the ſame manner in other Anemonies as it is ſeen here, or at leaſt does not always appear to be ſo. Figure A ſhews a mouth as engraved from another Anemone, but which alters or ſhifts its form every moment. This Anemone has got five rows of limbs. There are ten of them in the innermoſt row; the like number in the ſecond; twenty in the third; thirty in the fourth; and fourſcore in the fifth.

When

Dans la figure 10 de la même planche ſe voit une anemone de mer, de la ſeconde eſpece, cachée dans le ſable, et couverte en divers endroits de coquillages briſés, et de gravier, dont cet animal ſe fait une eſpece de côte d'armes, ſous laquelle elle ſe cache, mais dont elle peut ſortir en un inſtant. La figure la repréſente dardant de l'eau de ſa bouche, et de l'extrémité d'un de ſes membres.

La figure 11 de la XVII. planche montre la même anemone ouverte. La bouche eſt au centre de la partie ſupérieure: cette bouche n'eſt pas toujours formée de la même maniere dans d'autres anemones, ou du moins ne le paroit pas toujours. Vous voyez en A une bouche deſſinée d'après une autre anemone, et qui à chaque inſtant change de forme. Cette anemone a cinq rangs de membres: le rang intérieur et le ſuivant en ont chacun dix; il y en a vingt dans le troiſieme, trente au quatrieme, et 80 au cinquieme.

Quand

When the animal is out of the water, and is ſqueezed, it ſpurts out water at the mouth and at ſeveral of its limbs at the ſame time; ſo that it imitates pretty well the play of water-works. When the limbs are drawn in cloſer together, they give it the look of a flower, eſpecially of an Anemone.

Fig. 12. ſhews an Anemone of the ſame ſpecies, turned inſide out, as when a purſe or ſtocking is ſo. A thin tranſparent membrane, with white ſtripes, lines the whole inſide of the animal; and through it are ſeen the bowels, part of which hang or come out at the middle. One may obſerve, beſides, in this figure, two hollows ſinking in, which are formed by two pretty ſtrong cartilages.

-- Quand l'animal eſt hors de l'eau, et qu'on le preſſe, il jette de l'eau par la bouche, et pluſieurs de ſes membres en même tems, imitant aſſez bien ainſi le jeu des jets d'eau. Lorſque les membres ſont ſerrés l'un ſur l'autre, ils donnent à l'animal la figure d'une fleur, et ſurtout d'une anemone.

A la figure 12, de la même Planche, vous voyez une anemone de la même eſpece, mais tournée du dedans en déhors, comme un bas ou une bourſe. Tout le dedans eſt revêtu d'une membrane fine et tranſparente, avec des rayes blanches, les inteſtins paroiſſent au travers, et une partie ſort du milieu et pend au déhors. On voit auſſi deux ouvertures, qui penetrent dans l'intérieur, et ſont compoſées de deux cartilages aſſez forts.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 5.

Fig. 6.

Fig. 8.

Fig. 9.

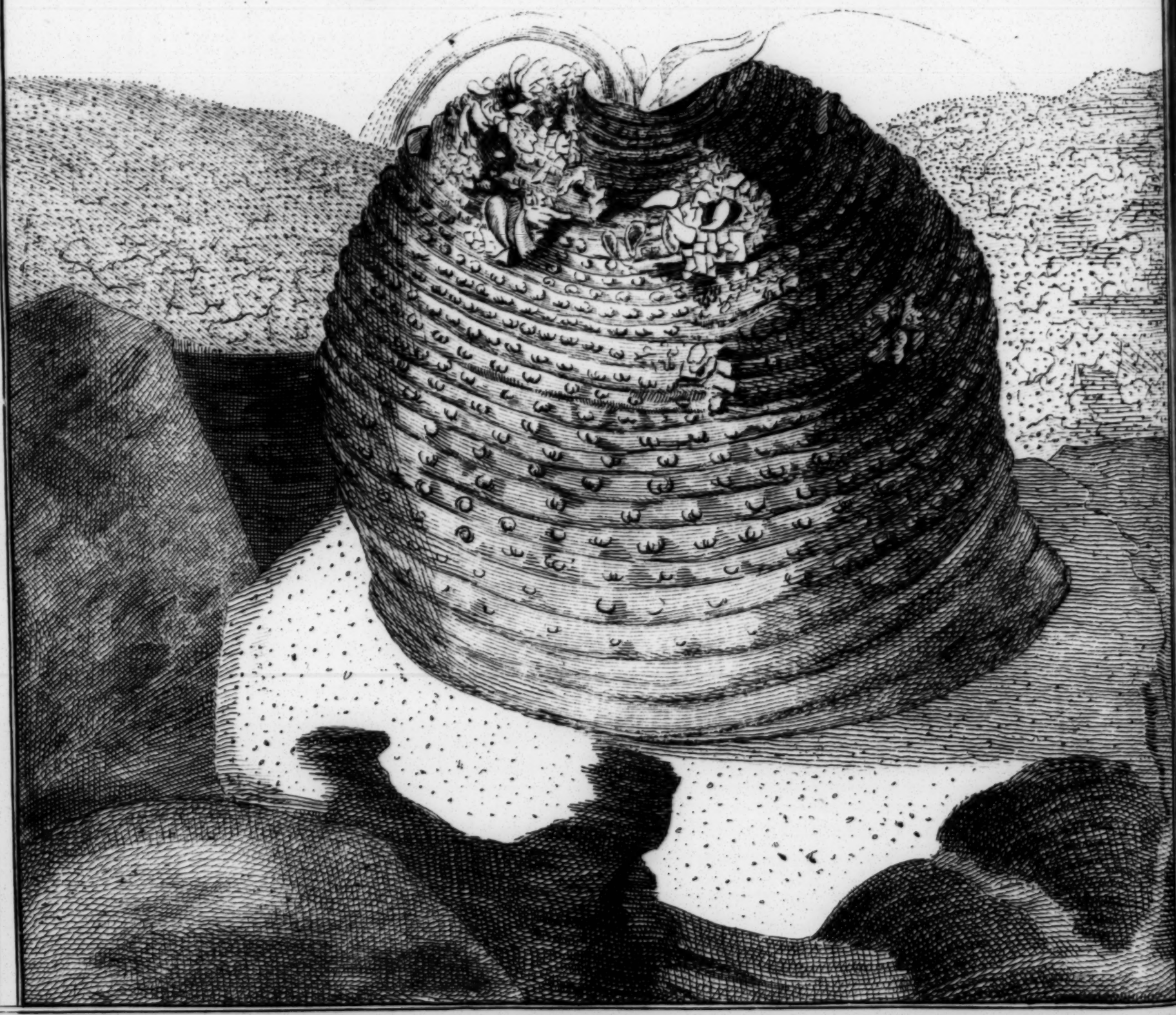
Fig. 7.

Fig. 13

Fig. 14

PICQUEMARE *del.*

Fig. 10.



J. Basire Sculp.

Fig. 1.

Fig. 2.

Fig. 3

Fig. 5.

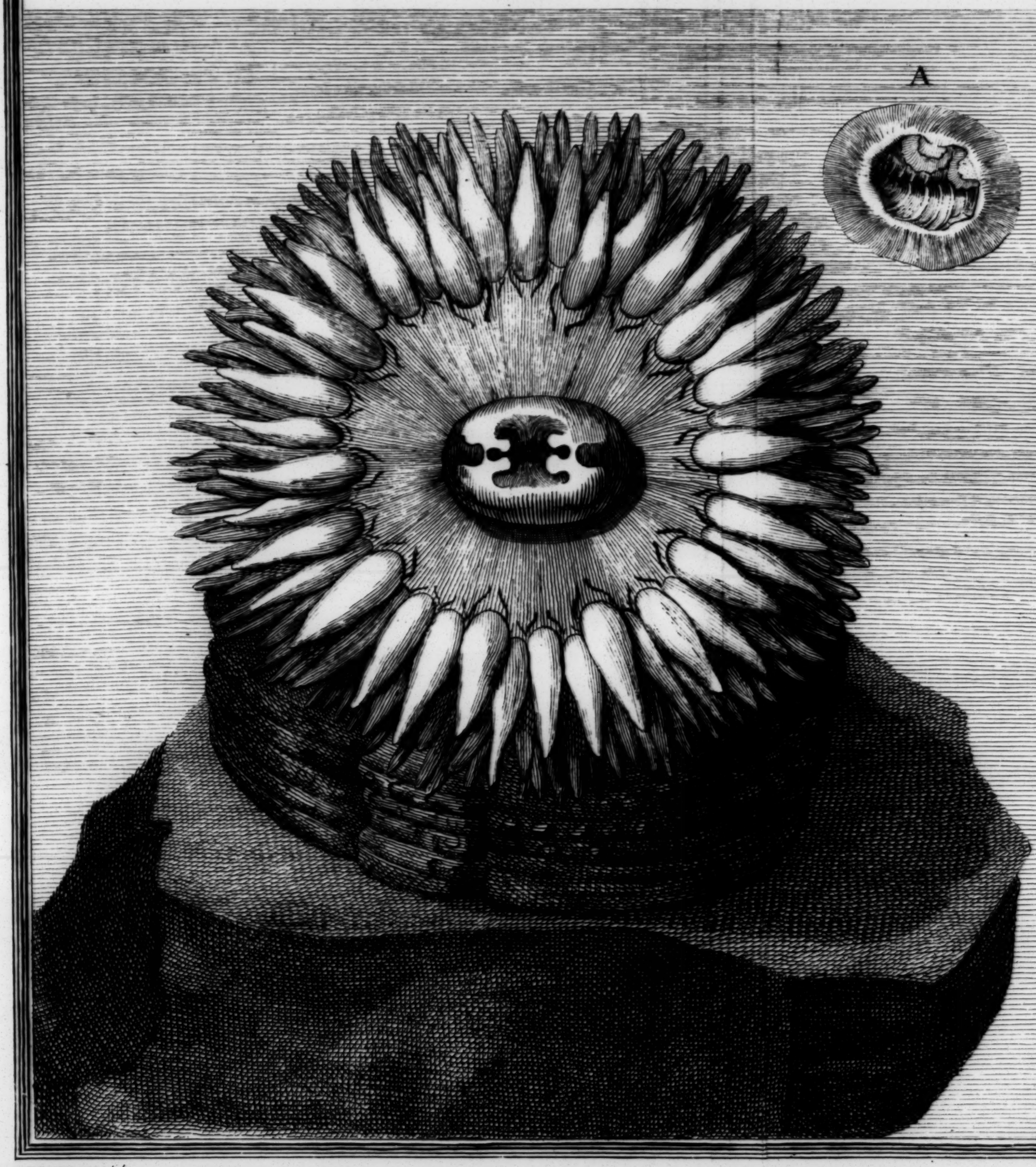
Fig. 6

Fig. 13

Fig. 14

PICQUEMARE *del.*

Fig. n.



DUQUMARE del.

Fig. 12.

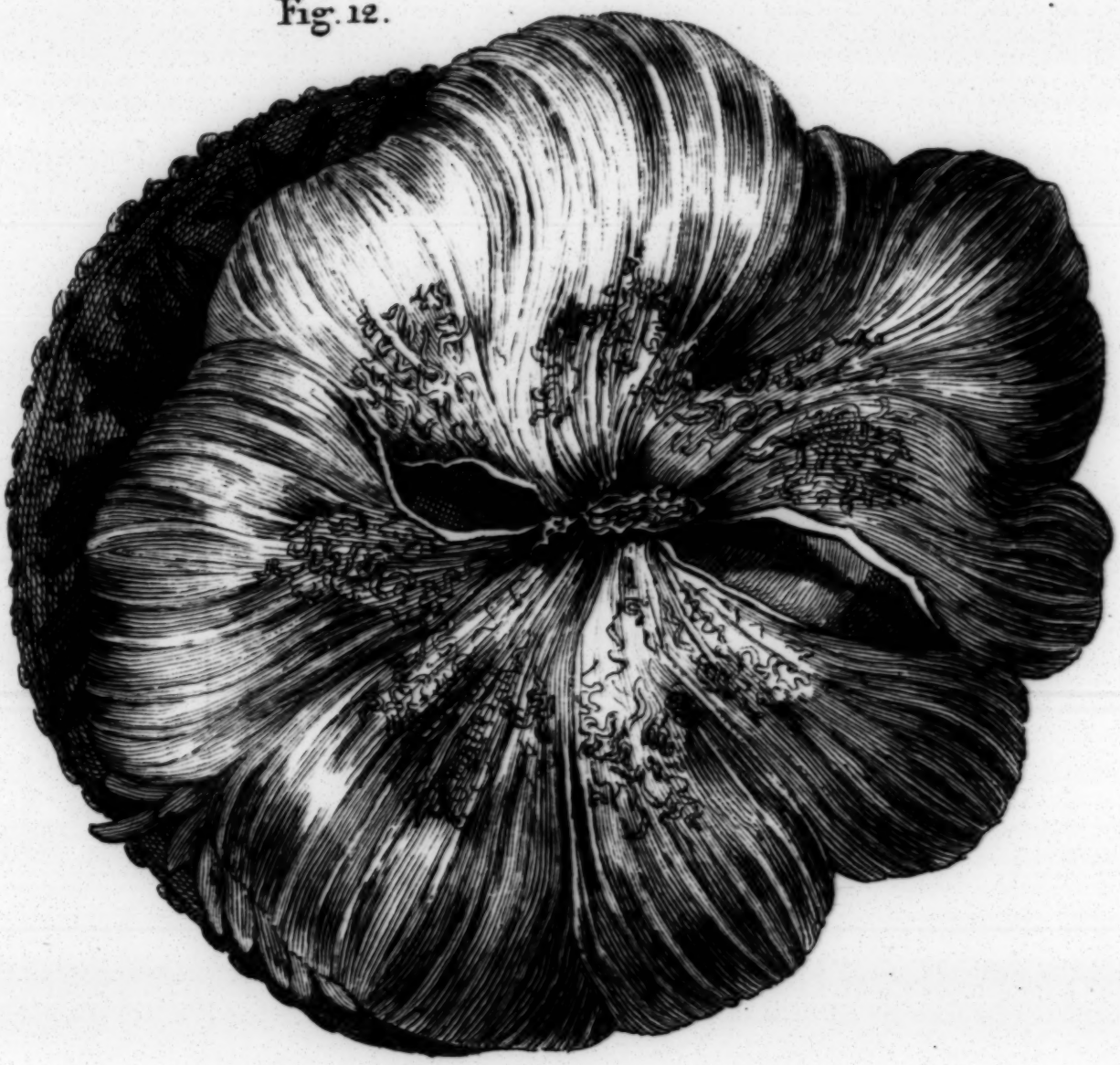
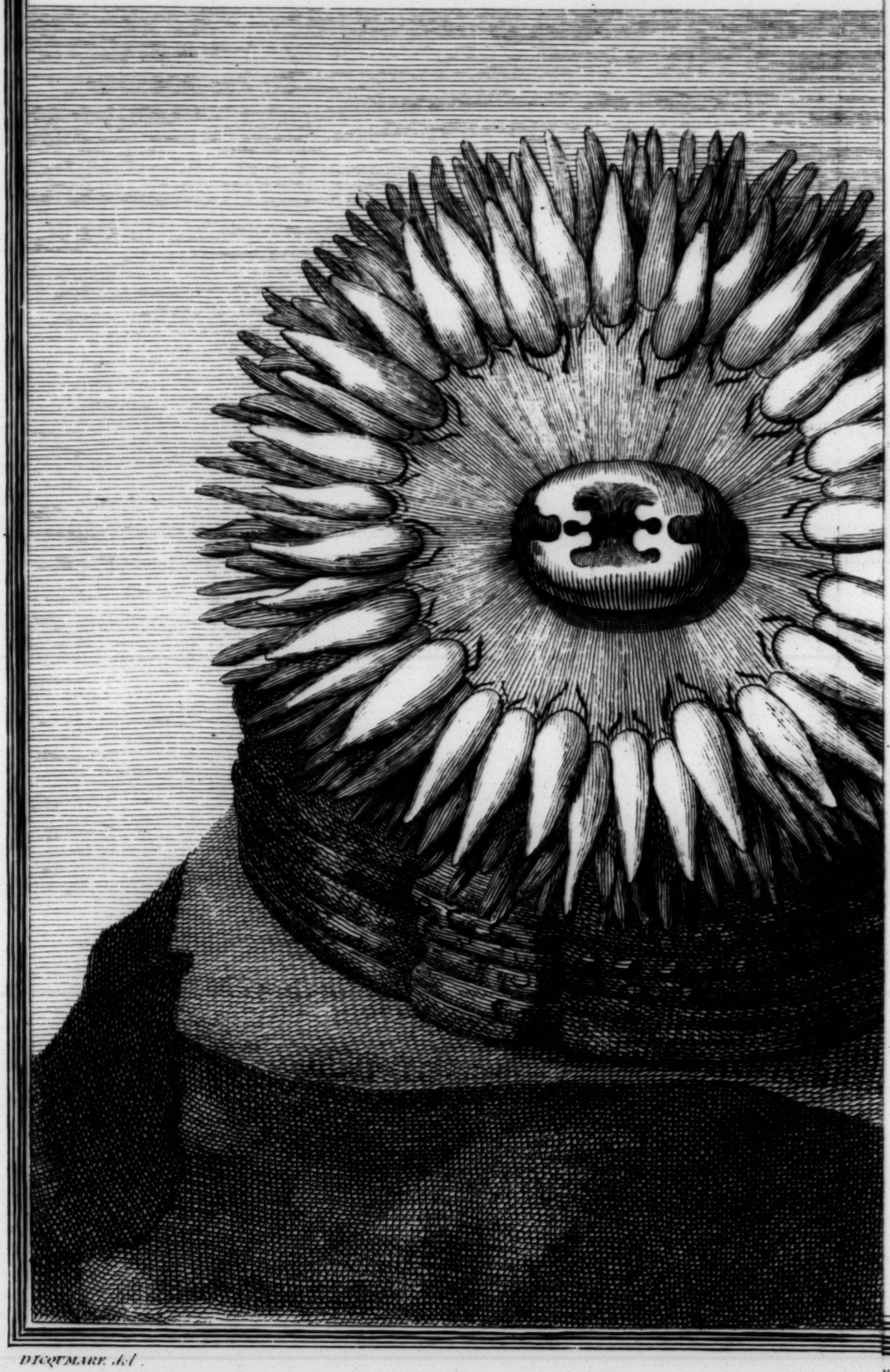


Fig. n.



DUCQUMARE del.

N.B. Dr. Solander, F.R.S. being conſulted about theſe ſea-worms, which are evidently of the claſs of the Actinia, referred the firſt ſpecies, Tab. XVI. Fig. 1—3. to the *Actinia Equina* Linn. Syſt. Nat. 1088. 1. the ſecond ſpecies, Tab. XVI. Fig. 10. and T. XVII. Fig. 11 and 12. to the *Actinia Senilis*. Ib. 1088. 2. and the third, Tab. XVI. Fig. 13 and 14. to the *Actinia felina*. Ib. 1088. 3.

XXXVIII. *Account of a new Hygrometer.* *By* M. J. A. De Luc, *Citizen of* Geneva, *F. R. S. and Correſpond. Member of the Academies of* Paris *and of* Montpellier.

Read June 10, 1773.

IN laying before the Royal Society an account of my attempts to find out a method for meaſuring the moiſture of the air, I think myſelf obliged to relate the gradual ſteps of my mind, the obſtacles I met with, the means by which I endeavoured to overcome them, the degree at which I flatter myſelf to have arrived, the hopes that may be entertained of farther advances, and the uſes which may be derived from my firſt experiments.

Attempts to invent an HYGROMETER.

1. In order to proceed regularly in this inveſtigation, I began by examining the eſſential requiſites in a machine intended to meaſure humidity, which I found to be the three following:

1ſt, The ſettling of a fixed point, from which every meaſure of the ſame kind ſhould be taken, ſuch, for inſtance, as that of boiling water in a thermometer, when the barometer is at a certain height.

2d, Degrees equally determined, or comparable, in different hygrometers, ſuch as are in the thermometer,

meter, the ſcales of Fahrenheit, Deliſle, Reaumur, &c.

3d, Conſtancy in the variations produced by the ſame differences of humidity.

2. I perceived, moreover, that it were to be wiſhed, that the hygrometer ſhould give a true indication of the relation between the real quantities of the humidity, or at leaſt between their differences: but this laſt point I rather conſidered as a deſirable degree of perfection, than as an indiſpenſable requiſite; the eſſential point being, that obſervers might underſtand each other, when mentioning degrees of humidity; and this ſeemed to be ſufficiently provided for by the abovementioned conditions.

3. Having thus planned to myſelf the work I had to go through, I firſt attended entirely to the firſt point, and laid aſide all the others. This I again ſubdivided. I had ſoon perceived that I muſt begin by thinking much leſs of the hygrometer than of the different phænomena of humidity. For this purpoſe it was neceſſary to find out a fixed ſtate, either of bodies in general, or of ſome body in particular; and this fixed ſtate might either be extreme humidity, or dryneſs, or any intermediate point.

4. Knowing that the extremes in nature are commonly very difficult, and ſometimes impoſſible to hit, I conceived at firſt greater hopes of intermediate degrees. But in vain did my imagination fatigue itſelf in a road, which I was forced to abandon.

5. I then came to the extremes, and that of abſolute dryneſs was the firſt I was induced to try. But having found no other way to procure it but by fire, and fire not producing it in all bodies which appeared

to me ſuſceptible of humidity, but by altering their nature, I reluctantly perceived that I ſhould be obliged to look for my firſt point, where I had the leaſt hopes of diſcovering it.

6. I remained a long time without diſcovering any thing in this new road; and very often turned back, but was always obliged to return to extreme humidity, as to the only part of my object, of which I could poſſibly get any hold.

7. The words, which are neceſſary for communicating our ideas to others, are often obſtacles to the raiſing of new ideas in ourſelves. They are by far too few to expreſs diſtinctly every ſhade of intellectual objects. Humidity was a word which I conſtantly repeated to myſelf, and it conſtantly led me to a claſs of phænomena, in which I could find nothing ſettled.

8. Water at length preſented itſelf to my mind; and in this fluid, which to all appearance ought firſt to have ſtruck me, I beheld with ſurprize, what I had been labouring, through many a round, to diſcover, under the denomination of extreme humidity. I was not at that time conſidering humidity in any particular phænomenon; I only obſerved that it was conſtantly produced by aqueous particles diſſeminated through bodies; and I found in water the maximum of the approach, and conſequently of the action, of theſe particles.

9. In order now to avoid the ambiguities from whence, in my opinion, the difficulties in theſe matters ariſe, let me be allowed for the future to employ no words but ſuch whoſe meaning is well determined. Humidity will accordingly be no more

that

than an effect, or modification of bodies from a ſubſtance more or leſs abundant, but conſtantly conſiſting of aqueous particles under different forms. This ſubſtance, conſidered in its utmoſt extent and under all the appearances which it aſſumes in nature, I ſhall expreſs by the Latin word *humor*. Thus ice, water in its different degrees of heat, hail, ſnow, icicles, rain, dew, clouds, fog, miſt, inviſible vapours, are no more than modifications of this ſame ſubſtance, different ſpecies of a determined genus; ſince aquoſity, which is common to all, is its generic character.

10. The more humor there is in any body, the more humid that body is; and conſequently if it be plunged in water, and ſoaked ſo as not to be able to receive any more, it is got to extreme humidity, the water which fills up all its pores being humor in the higheſt degree of intenſity.

11. Not, however, but that diſcrete humor, or vapour of every kind, may in ſome reſpects produce as great effects as concrete humor or water: but there is always ſome difference in ſome other reſpect, and chiefly in regard of time. Bodies encompaſſed with air are continually diſcharging, by evaporation, part of the humor they imbibe from it. If the circumſtances are ſuch, that the humectation exceeds the evaporation, the body at length wets through *, more or leſs quickly as the quantity of humor which it receives in a given time is greater or leſs, and likewiſe in proportion as this quantity exceeds that

* By *wetting* here, I underſtand arriving at the greateſt degree of humidity.

which

which evaporates. It is ſuddenly wetted, when the humor is ſo condenſed as to become water, becauſe the evaporation which takes place at the ſurface of the water, does not weaken its action on the bodies dipped into it; it is only wetted little by little, or, what happens oftenеſt, in part, when the humor is diſcrete or reduced into vapour; becauſe, while it is depoſited on particular ſpots, it evaporates from the interſtitial parts, and that more or leſs according to the ſtate of the air, and that of the moiſtened bodies.

12. This difference, however, in point of time, between the action of concrete and diſcrete humor, only takes place on the ſurface of bodies, or at a ſmall depth; it diminiſhes, and may even become oppoſite, as the depth of bodies increaſes, becauſe the diſcrete humor is then more eaſily introduced into their pores than water, which more than makes up for their different intenſity.

13. This conſideration ſolves a difficulty, which at firſt puzzled me. I had been told by bird-catchers, that the threads of thoſe nets which they caſt on the water-ſide, were leſs ſtretched from the action of water than of dew. Hence it might ſeem, that what I took for the extreme of humor had leſs effect than what is only a degree of it. But two particular cauſes accounted for this difference.

1ſt, The air contained within the fibres of the thread oppoſes the introduction of the water, which, preſenting itſelf in a body, ſhuts up the paſſages by which the air ſhould eſcape to give it room; but it yields to the drops of dew, which permit its eſcape while they penetrate through the threads.

2d, Another

2d, Another particular cauſe, leſs obvious though not leſs probable than the preceding, is the difference in the mutual attraction of parts, in the concrete and diſcrete humor, and conſequently in their reſpective facility to ſeparate, and get one by one through the narrow pores. When this entrance is attempted by the humor, under the form of water, the mutual attraction of its parts, being greater than in dew, occaſions a greater reſiſtance to their introduction, than when they are already divided by ſome other cauſe, viz. when the humor is reduced to ſmall drops, or vapor.

14. This phænomenon, therefore, does not contradict my principle; it is only a particular fact; and it remains true, that bodies ſurrounded with water are expoſed to the extreme of humor. To remove this cauſe of exception from my hygrometer, it was ſufficient to provide outlets for the air, and not to increaſe too much the thickneſs of the body, upon which the humor was to act.

15. Another difficulty, which preſented itſelf, was that water might probably act with more or leſs energy in proportion to its heat. But this did not ſtop me long. As my preſent object was a fixt point for the hygrometer, and not the greateſt power of water, conſidered as a cauſe of humidity, it was enough to employ it conſtantly at the ſame degree of heat; and, to fix this with greater preciſion, I determined to uſe water at the inſtant that it ceaſes to be ice. The baſis therefore of my hygrometrical ſcale was to be the ſoaking power of melting ice.

16. This principle, being thus unfolded, appeared ſo ſimple, that I was at firſt ſurpriſed how it could have

have been ſo long overlooked. But I afterwards accounted for this, from the difficulties which I met with in the diſcovery. The notion of an hygrometer being both complex and unſettled, all the obſtacles preſented themſelves at once, and this multitude of ideas exceeded the power of attention. The very firſt ſteps were apt to miſlead. On the one hand, I looked for an hygrometer with a head full of the matters already uſed for hygroſcopes, which always are more or leſs altered by water; and, on the other, the name of humidity was applied to that cauſe, the effects of which I wiſhed to meaſure; and both points of view turned aſide the mind from the idea of water, as being proper to afford the required fixed point in an hygrometer.

17. The firſt difficulty had not eſcaped me; but, conſidered in itſelf, it did not appear unſurmountable. I was in hopes that a ſubſtance might be found capable of being affected by the ſoaking power of water, without being altered by it. As the nature of this ſubſtance was to determine not only the form of the hygrometer, but alſo the ſpecies of the degrees, which were to indicate the different quantities of humor, I concluded that my ſecond object ought to be the diſcovery of this ſubſtance.

18. In this reſearch, I again divided the objects, by conſidering ſeparately the three kingdoms, viz. the mineral, the vegetable, and the animal. The two firſt offered no ſubſtance fit for my purpoſe, viz. none that would obey the impreſſions of humor, without being altered either by it or by other cauſes. But in the animal kingdom bones drew my attention; and ivory, in particular, ſeemed to poſſeſs the

required

required qualities. I had obſerved that the key of an ivory cock was tighter or ſlacker, as there was more or leſs humor in the air. Ivory pallets, uſed for water-colours, ſhewed no alteration, at leaſt none that was laſting. I knew alſo the elaſticity of this ſubſtance, which ſeemed to ſecure its coming back to the ſame ſtate, on its return to the ſame degree of moiſtneſs.

19. There ſtill remained on this ſecond head another object of inquiry, which was almoſt neceſſarily connected with the third, viz. the ſpecies of the degrees to be given to the hygrometer. The beſt form to be given to the ivory, in order to receive with eaſe the impreſſions of the humor, and to have its effects meaſured upon it, was to be determined. I firſt thought of ivory rods, the lengthening of which ſhould be meaſured by a machine ſimilar to the pyrometer. I likewiſe had ſome notion of a large nonius, formed of an ivory and a metallic rod. Either of theſe machines would admit of a fixed graduation, as both the dimenſions of their parts and proportions to one another could be determined. But then I apprehended that ivory might perhaps, like wood, have its longitudinal fibres but little liable to be extended by the humor, and that the imperfections of theſe two kinds of micrometers would occaſion a conſiderable irregularity in the hygrometrical degrees. I alſo feared that if ivory rods were made thick enough to prevent their bending, ſuch a thickneſs might become an obſtacle to their intire penetrability by the humor (14). I therefore concluded that the ivory ſhould have ſuch a form, that, though very thin, it might not warp; and that the meaſurable varia-

tions were to be the removal or approach of its fibres to each other.

20. Being thus guided by theſe neceſſary conditions, I thought of different thin ivory cups, the capacities of which ſhould be meaſured by quickſilver; and at laſt imagined a hollow cylinder, in which the variation of its capacities, when more or leſs moiſt, might be meaſured by the quickſilver it ſhould be filled with; and which putting into a glaſs tube joined to the ivory one, would of courſe riſe more or leſs, as that veſſel was more or leſs deprived of humor.

21. Nothing now remained but to find out a way of eſtimating the changes of capacity of the ivory tube, by means of the variations in the height of the mercury in the glaſs tube. I thought, at firſt, that by uſing very nice ſcales, in order to compare the weights of the mercury contained in the cylindrical veſſel, with that of a column of the ſame liquid in the tube, I might obtain the proportions of theſe weights with a ſufficient exactneſs, to be able to meaſure the variations of the mercurial column, by degrees repreſenting aliquote parts of the whole maſs.

22. This in itſelf was undoubtedly an exact method; but then it required in the execution ſuch a nicety in the ſcales, that I durſt not employ it in the conſtruction of an inſtrument of ſo extenſive an uſe. Such ſcales are always ſcarce from their high price. I remembered to have myſelf found that inconvenience in the conſtruction of a Deliſle's thermometer, and concluded I muſt hit upon ſome method to avoid it.

23. The idea of a thermometer, which ſtruck my mind, was a lucky one. I was led to it by a kind

kind of connexion between the ſcale of that inſtrument and that of my hygrometer. I ſoon perceived, that, by applying to my hygrometer a thermometrical tube, already graduated by means of two fixed points of heat, it would only be neceſſary to know the proportion of the weights of mercury in this thermometer, and the hygrometer, to which its tube was to be applied, to have in this laſt inſtrument degrees as well determined as in the firſt. Scales of a common degree of exactneſs were ſufficient to eſtabliſh between the reſpective degrees of both inſtruments a proportion equal to that of their mercurial weights (42 and 43).

24. Beſides the eaſe in the execution, this contrivance afforded me a very ſimple method to correct the effects of heat upon the mercury contained in the hygrometer. It is indeed obvious, that, abſtractedly of the effects of the humor, the new inſtrument muſt in itſelf be a regular thermometer; and that conſequently the variations of an adjoint thermometer were immediately to point out this correction.

25. Every principle being thus ſettled, nothing remained but to contrive its conſtruction. I began by making ſome experiments, on the nature and quantity of the action of water upon ivory. I made for that purpoſe a ſmall cylindrical ivory veſſel, of an inch in diameter, and eight lines in length, and reduced its thickneſs to leſs than $\frac{1}{4}$ of a line. I likewiſe prepared a wooden cylinder, equal in its diameter to the internal one of the veſſel. I then put this veſſel into water, in ſuch a manner that it only wetted it outwardly to the rim. In a very ſhort time the wooden cylinder, which at firſt filled the

cup exactly, no longer filled it. After a few hours, I perceived that the internal ſurface grew wet, and by means of a magnifying glaſs, found it covered with a very fine dew. This dew did not encreaſe by the veſſel remaining any longer in the water; the evaporation being doubtleſs equal to the tranſudation; and the capacity of the veſſel, which encreaſed till the appearance of the dew, ſeemed afterwards at a ſtand.

26. This tranſudation puzzled me a little; it ſhewed me that the water would get into my hygrometer, which at firſt appeared an inconvenience. I ſoon, however, found an advantage in it. The water, after having ſoaked through the ivory, would immediately puſh back the mercury, which having by degrees ſunk in the tube, during the penetration of the water through the pores of the ivory, muſt thus riſe again. Hence I might expect a maximum for the fall of the mercury, very eaſy to be determined. As for the water introduced into the ivory veſſel, I was in hopes that it would go back as ſoon as the outſide of the cup ſhould be dry.

27. Having thus aſcertained that ivory was very eaſily affected by the impreſſions of the humor, it ſtill was neceſſary to know, whether the variations of the one would always equally anſwer to thoſe of the other. Having accordingly taken my ſmall cup out of the water, and expoſed it to the air, I ſoon found that its capacity diminiſhed, but that even after ſeveral days it did not return to its former ſtate. This again puzzled me; but I ſuſpected that the external preſſure of the tool upon the ivory might ſomewhat have compreſſed it, and that the water having reſtored the ivory fibres to their original pitch, the abſolute

abſolute capacity of the cup remained larger than it was before.

28. To ſatisfy myſelf about this, I got another wooden cylinder, which filled the capacity of the veſſel in its preſent ſtate. This I again put into the water, and left it there a ſufficient time; I then expoſed it to the air to be dried; and after that found that the wooden cylinder filled it as before. Hence I concluded, that in the conſtruction of my hygrometer the ivory cup ſhould be dipped for ſome time in water, and afterwards dried, before it was ufed.

29. Thus having cleared up my conjectures, as much as they could be, by theſe preliminary experiments, and got ſome inſight into the proportions of the different parts of the machine, I proceeded to its conſtruction, and finiſhed it in the following manner:

Deſcription of an HYGROMETER.

30. Tab. XVIII. figure 1. ſhews the ſection of the interior part of the inſtrument, of its true length, in the direction of its axis.

The firſt part to be deſcribed, being in ſome meaſure the ſoul of the hygrometer, is an ivory tube, *a a b*, open at the end *a a*, and cloſed at *b*. It is made of a piece of ivory, taken at the diſtance of ſome inches from the top of a pretty large elephant's tooth, and likewiſe at the ſame diſtance from its ſurface, and from the canal which reaches to that point (68). This piece is to be bored exactly in the direction of its fibres; this hole is to be very ſtreight, and its dimenſions are $2\frac{1}{2}$ lines in diameter, and two inches 8 lines in depth from *a a* to *c*.

31. Prepare

31. Prepare after this a braſs cylinder, about $3\frac{1}{4}$ inches long, and to one of its extremities fix the pully proper to receive the ſtring of the bow when the piece is turning. This cannot be done too carefully, both to make it perfectly round, and to fit it exactly to the hole of the ivory tube; its extremity muſt even be rounded, that it may be applied cloſely to the bottom of the hole. Having then roughly prepared the outſide of the ivory tube, and introduced into it the braſs cylinder, put both pieces thus united upon the turning wheel, and find out on the outſide bottom of the ivory tube, the point which anſwers to the axis of the braſs piece, in order that this may turn exactly upon its axis. It is with this view that the braſs cylinder is made longer than the ivory tube.

32. All theſe precautions are deſigned to make the ſides of this tube of an equal thickneſs, viz. $\frac{3}{16}$ of a line, except at the two extremities. At the bottom *b* the tube ends in a point, and at the top *a a*, it muſt for about two lines be left a little thicker, in order to enable it to bear the preſſure of another piece, which is to be put into it. Thus the thin or hygrometrical part of the tube will be reduced to $2\frac{1}{2}$ French inches, including the concavity of the bottom.

33. Before this piece is uſed, put it into water ſo as that the external part alone be wetted by it, and leave it there till the inſide be every where covered with the dew I mentioned before (25). This will take place in a few hours; I have given the reaſons for this operation (28).

34. The glaſs tube intended for this hygrometer muſt be about 14 inches long. Its lower end is ſeen

in

in *d d e e* (fig. 1.). Its internal diameter is about $\frac{2}{3}$ of a line. The reaſon why it ſhould not be ſenſibly leſs will be given hereafter (52); and if it was ſenſibly larger, the variations of height in the mercurial column would not be conſiderable enough. On the dimenſions that I propoſe, when the hygrometer is put into melting ice, in a fine ſummer day, the mercury falls about ſix inches in the tube. The outſide diameter of this tube ſhould be about two lines, in order that the part *g g* of a braſs piece through which it paſſes, and which is to enter into the ivory pipe, be as thin as poſſible.

35. The glaſs tube, as I ſaid before, ſhould have belonged to a thermometer. Its extremity widens of courſe towards the ball; which will be of uſe, when the mercury is poured into the hygrometer, in order that it may drive the air before it, by riſing from the ivory pipe into the glaſs tube. To preſerve this widening, break the ball of the thermometer by ſtriking againſt the bottom; and with pinchers take off the reſt little by little, and make the extremity cylindrical by grinding it upon the wheel. The ſame muſt be done at the top, which I ſuppoſe to have been made to end in an olive or ſmall reſervoir for the filling of the thermometer. This widening is likewiſe to be ſaved for the reaſons hereafter to be mentioned (52).

36. The piece *f f g g* is intended to join the ivory with the glaſs tube. It is of braſs, ſhaped as in the figure. A cylindrical hole is bored through it, which holds the glaſs tube as tight as poſſible, without danger of breaking it; and its lower part is to enter with ſome degree of difficulty into the ivory pipe.

37. To

37. To hinder the part of that tube, which encloſes the braſs piece, from being affected by the variations of the humor, which might ſometimes prevent a ſufficient preſſure, I cover this part of the tube with a braſs verrel, repreſented in *h h i i*. It muſt enter with force, and will henceforth be conſidered as part of the ivory pipe.

38. To unite thoſe pieces together, I make uſe of gum lac, or of maſtich, which melts by the heating of the glaſs and the braſs. I firſt cement the braſs piece with the glaſs tube by introducing the tube, and leaving it at firſt at an inch diſtance from the place where it is to be fixed; I then hold this end of the tube over live coals, by bringing it nearer and nearer, and turning it, that both that and the braſs piece be every where equally heated; and when they are hot enough to melt the gum lac, I rub the glaſs tube with it, and puſh the braſs piece to its place by means of a hollow bit of wood, drawn beforehand over the tube for this purpoſe. As the braſs piece advances, the lac accumulates towards the end of the tube; I take away the ſuperfluous part, but leave a ſlight coat of it over the end of the braſs piece, in order to preſerve it from the contact of the mercury that might corrode it. When this piece is properly placed, and ſtill warm, I cover with lac its cylindrical outſide, and introduce it into the ivory tube, which has been ſomewhat warmed by holding it near the fire, in order that the lac may ſtick more cloſely to it. As ſoon as theſe pieces are cold, they are found very ſtrongly cemented together, and neither mercury nor water can make their way between them.

39. The introduction of the mercury is the next operation. I firſt roll a ſlip of paper three inches wide over the glaſs tube, and tie it faſt to the extremity which is neareſt to the ivory pipe. I then introduce into the tube a horſe hair long enough to enter the cylinder by one end, and to have the other riſe three or four inches beyond the orifice of the tube. I then raiſe the paper which has been ſhaped round the tube, and uſe it as a funnel to pour the mercury into the inſtrument, which I hold upright. The pureſt quickſilver ought to be employed for that purpoſe, and it will therefore be proper that it ſhould be revivified from cinnabar. I poured it then into the paper funnel, from whence it eaſily runs into the tube, with the aſſiſtance of ſome gentle ſhakes. The air which it drives before it comes out along the horſe hair. Freſh mercury muſt from time to time be ſupplied, to prevent the entire emptying of the paper tube, and the running in of the mercurial pellicle, which the contact of air always produces upon the ſurface.

40. Some air bubbles generally remain in the tube; they may be ſeen through the ivory pipe, which is thin enough to have ſome tranſparency. Theſe being collected together by ſhaking, muſt be brought to the top of the tube, and expelled, by means of the horſe hair. To facilitate this operation, ſome part of the mercury muſt be taken out of the tube, in order that the air may be leſs obſtructed in getting out, and the horſe hair have a freer motion to aſſiſt it.

41. Air, however, cannot be entirely driven out in this manner. It is the weight of the mercury,

with which the tube is for that reaſon to be filled, that in time completes its expulſion, by making it paſs through the pores of the ivory. To haſten this, I place my hygrometers in a box made on purpoſe; and this I fix pretty nearly in a vertical direction, to the ſaddle of a horſe, which is ſet a trotting for a few hours. The ſhakes ſometimes divide the column of mercury in the glaſs tube, but it is eaſily reunited with the horſe hair. When, upon ſhaking the hygrometer vertically, no ſmall tremulous motion is any longer perceived in the upper part of the column, one may be ſure that all the air is gone out.

42. I now come to the operations requiſite to make the ſcale of the hygrometer, and firſt of all to that which determines the baſe (15). This may be done as ſoon as the air is gone out. I then ſuſpend the inſtrument in a veſſel filled with ice mixed with the water it produces in melting. I take care to ſupply the melting by recruits of freſh ice, during the courſe of this proceſs, which laſts ten or twelve hours. In the firſt hour, the mercury ſinks above one third of the ſpace it has to go through; it advances leſs in the ſecond; and its motion leſſens thus gradually, till it appears ſtationary, which frequently happens after ſeven or eight hours, and it remains two or three hours in that ſituation. The ivory being then become more tranſparent on account of humidity, a very thin dew is perceived by a certain play of the light on the ſurface of the quickſilver. Laſtly, the mercury begins to reaſcend; the operation is terminated; and ſmall drops of water, as I expected, are at that inſtant ſeen upon its ſurface (26).

43. I follow the laſt ſteps of the mercury in its fall, by means of a fine ſilken thread fixed very tight around the tube, This is left at the loweſt point it has been brought to. If this point be too low, relatively to the frame of the hygrometer, freſh mercury is poured in, and the thread proportionally drawn up higher; if too high, I take off ſome of the mercury and lower the thread; and in both caſes make uſe of the horſe hair. This muſt be done when the mercury ceaſes to fall, in order that the place where the thread is to remain may be immediately determined by this operation.

44. This point thus fixed is named o in my hygrometer; it is that in which dryneſs is nothing (if I may be allowed to expreſs myſelf ſo), ſince it is that of extreme humidity, in a given heat; viz. that of melting ice. From this point are reckoned all the degrees I am now going to ſpeak of; which thus become degrees of exſiccation.

45. The laſt eſſential operation is that by which the ſize of the hygrometrical degrees are determined; and this I ſhall deſcribe by an example. It muſt be remembered that the hygrometer's tube was originally a thermometer (23). I take it in this firſt ſtate, in the inſtance I am going to give. The diſtance between the thermometrical points of melting ice and boiling water, at twenty-ſeven French inches of the barometer, was found to be 1937 parts of a certain ſcale. I broke the bulb of this preparatory thermometer, in a baſon, in order to receive carefully all the mercury that it contained. This being weighed in nice ſcales, amounted to 2 *on.* 11 *dr.*

12 *gr.* or 1428 grains. All the pieces of my hygrometer being put together, it weighed 373 grains, and when filled with the proper quantity of mercury 833. It conſequently contained 460 grains of mercury.

46. By the rule above given (23), the extent of the hygrometer's degrees, ought to be to that of the degrees in the preparatory thermometer, in proportion of the reſpective weights of mercury in the hygrometer and thermometer; and conſequently as the weight of the mercury in the thermometer is to the weight of the mercury in the hygrometer, ſo is any given interval in the thermometrical ſcale, to the correſponding interval in the ſcale of the hygrometer. Conſequently in our example as 1428 : 460 :: 1937 : 624 (nearly); and the correſponding intervals on the ſcales of the thermometer and the hygrometer, ought to follow the proportion of 1937 to 624.

47. I call the diſtance between the two fixed points of heat in the thermometer the *fundamental interval*; and I ſhall call the *fundamental line* in the hygrometer that of which the length correſponds to this interval. Thus the *fundamental interval* in the preparatory thermometer, being 1937 parts of a certain ſcale, the *fundamental line* of my hygrometer conſiſted of 624 parts of the ſame ſcale. This example may ſo eaſily be applied, that it will be unneceſſary to dwell any longer upon this ſubject.

48. Having thus got a *fundamental line* in the hygrometer, I had it in my power to divide it into as many parts as I thought proper: my choice was naturally

naturally to be determined by the ſimplicity of a proportion between the degrees of the thermometer, and thoſe of the hygrometer, becauſe this laſt was to be corrected by the firſt, on account of the effects of heat (24). My firſt thought was to divide this *line* into 80 parts, agreeably to the diviſions of the fundamental interval in what I call the *common thermometer* in my book upon the air, which I ſhall always be underſtood to mean in this paper. But as the minuteneſs of theſe degrees was found to be inconvenient and ſuperfluous, I determined to make them double, by putting only 40 in the length of my hygrometer's fundamental line. It is eaſily underſtood that theſe degrees, thus ſettled, begin to be reckoned from the place of the thread, which indicates upon the tube of the hygrometer extreme humidity, by the heat 0 of the *common thermometer*, or of melting ice.

49. The inſtrument with its frame is ſeen fig. 2. the dimenſions of which are every way one half of thoſe of the original. It is mounted on deal, that being the wood, which ſuffers the leaſt change in the length of its fibres. The lower part of the frame is ſlit through the whole length of the ivory pipe, in order that the air may circulate freely round this pipe, and the bulb of a thermometer which I ſhall mention preſently. The hygrometer is faſtened in three parts; viz. at bottom on a ſmall bracket, at top by a tube paſſing through a piece either of hard wood or of metal faſtened by ſcrews; but chiefly by means of a braſs wire on the neck of the braſs piece, which unites the glaſs with the ivory pipe. This piece is laid in a ſmall plate of a hard wood,

wood, which in that place fills a groove originally made throughout the whole length of the lead-board.

50. To keep the duſt from getting through the opening of the tube, I ſhut it up in a ſmall ivory caſe. It cannot be ſealed up, becauſe if air was left in, it would obſtruct the riſing of the mercury; and if it was exhauſted, the mercury would be puſhed to the top by the preſſure of the atmoſphere upon the ivory pipe; as I have experienced it.

51. Hence however ariſes a ſmall inconveniency; which is, that as the upper part of the column of mercury communicates with the air, if it remains long in the ſame part of the tube, or moves but little in it, ſome dirt may be left on the ſides. This I eaſily remedy, by means of a braſs wire, the extremity of which is dentated in the form of a file, in order to hold ſome bits of cotton, which I put round it. The wire is eaſily introduced into the tube, by means of the widening mentioned before (35). I put it in, when the mercury is below the part it has ſoiled, and eaſily clean it by this means. It is on this account that the tubes to be employed are to be of about $\frac{3}{8}$ of a line internal diameter.

52. The ſcale of the hygrometer is marked upon a deal ſlip, which ſlides along the groove I mentioned before (49). This, as well as all the other parts of the frame, muſt be lined with paper, to mark the neceſſary ſcales; and this paper is afterwards varniſhed over. Thin plates of ſilvered braſs can be employed for the ſame uſe.

53. The mobility of the ſcale of the hygrometer ſerves to correct, in the obſervation itſelf, the effect

of the heat on the mercury. At the top of this ſcale is ſeen an index, over-againſt another ſmall ſcale, marked upon the unmoveable part of the frame. The degrees of this ſmall ſcale are eightieth parts of the fundamental line, and conſequently immediately anſwer to the degrees of the thermometer on the ſame frame (48). When the index points to o of the ſmall ſcale, the thread which indicates upon the tube of the hygrometer the point to which the mercury ſunk in the melting ice, anſwers likewiſe to o in the ſcale of the hygrometer. This is the caſe expreſſed in the figure wherein the thermometer is likewiſe repreſented at o of its ſcale. By firſt obſerving the heat therefore, and conducting the index to the point of the ſmall ſcale, which anſwers to the actual degree of the thermometer, the hygrometer will only indicate upon its ſcale the degrees of the humor. For this ſcale going through the ſame variations that the heat occaſions in the height of the column of mercury, the indications of the hygrometer become juſt the ſame as they would be, if the heat always continued that of the point at which extreme humidity was fixed, viz. o of the common thermometer.

The ſcale of the hygrometer is carried to the proper point, by means of a knob fixed on a ſmall piece of hard wood or metal, ſcrewed to the bottom of the board, and which affords a free paſſage to the tube of the hygrometer.

Account

Account of the firſt OBSERVATIONS *made on the going of this* HYGROMETER.

Read June 10, 1773. 54. My firſt hygrometer was ready for obſervation at the beginning of laſt February (1772), in a rainy ſeaſon. A few hours after it was taken out of melting ice it was already at 54 degrees of its ſcale. The next morning it was only at 50, but towards noon it roſe again to 54. I carried it down to my cellar, which being a conſiderable depth under ground is commonly very damp. As I went down the ſtairs, I perceived that my hygrometer continued falling, ſo that when I hung it up in the cellar it was as low as 35.

55. In the evening of the ſame day it was at $28\frac{1}{2}$, and the next night at $21\frac{1}{2}$. It continued falling imperceptibly during the reſt of this month, throughout the whole of the next, and till the 19th of April. On that day it was at $3\frac{1}{2}$, and conſequently very near extreme humidity: but in this interval it had very often rained, and ſnowed, and even when the ſky was clear over head, the ſtreets had always remained wet, ſo that it was evident from all the common appearances, that the humidity had gone on conſiderably increaſing in the cellar:

56. I was impatient to ſee the hygrometer riſe again in the cellar itſelf, which I could not however expect but with a north wind. At length, on the 20th of April, though the rain ſtill continued, the hygrometer roſe half a degree. In the night of the 20th to the 21ſt the wind came about to the north, and when I looked at my hygrometer in the morning, I found it at $6\frac{1}{2}$. It continued riſing imperceptibly

ceptibly the whole of that day, and the next morning ſtood at 9½.

57. Another circumſtance I was anxious to know, was, whether the hygrometer, after having been kept in the cellar ſo long, would riſe, upon being carried up ſtairs again, to the point from which it had fallen. The importance of this new obſervation prevented me from purſuing that I had begun in the cellar. I therefore took my hygrometer out of it, and while I was going up the ſtairs it roſe three degrees. This was at ſix o'clock in the morning. At ſeven it was already at 17, and at eight at 23½. From eight to eleven it roſe to 43, and at one o'clock ſtood at 63. After this it fell again, and at half an hour after five was no higher than 50. The ſky had been clouded during the laſt interval.

As the preceding obſervations relate only to the hygrometer, and not to humidity, I ſhall confine myſelf to them. They are ſufficient to give an idea of the going of the inſtrument in the ſeaſon they were made. I ſhall hereafter give an account of ſome obſervations taken at other ſeaſons.

FIRST EXPERIMENTS *made to diſcover the Accuracy of this* INSTRUMENT.

58. The moſt important thing after the preceding obſervations, was, to try whether the inſtrument was in reality comparable. To aſcertain this, I immediately conſtructed four more upon the ſame principles, which were finiſhed on the 23d of Auguſt.

59. I could not uſe my firſt hygrometer to make comparative obſervations with the new ones; its tube

being either too narrow or too ſhort. The proportion I had ſettled between the capacity of this tube, and that of the ivory pipe, was deduced from the preliminary experiments I had made in the month of December (29); and had of courſe been found juſt, as long as the ſpring laſted. But even before the new hygrometers were completed, the quickſilver had riſen in the firſt ſo as to run out of the top of the tube. This, joined to ſome other previous obſervations, which had convinced me that the diminution of the humor is much more conſiderable on mountains than in plains (76), induced me to fix the dimenſions of the tube of the hygrometer in the manner laid down in the deſcription of the inſtrument. I had been in time to follow theſe dimenſions in the conſtruction of my new hygrometers, ſo that when they were brought from extreme humidity to the ſtate of the air in my apartment in the month of Auguſt, the quickſilver did not riſe too high in them; that is, it remained ſufficiently below the top of the tube, to indicate leſſer degrees of humidity afterwards.

60. The four new hygrometers have been conſtructed with as little reference to each other, as if they had been made in different countries. By comparing them therefore, I have been enabled to judge of what might be expected from the agreement of inſtruments of this kind. This is what I have found.

When I have obſerved them in places where it appeared likely that the humor would be equally diſtributed among them, the utmoſt of their difference has been uſually from 19 to 21. Their greateſt height, for inſtance, in my room with the windows

ſhut,

ſhut, has hitherto been 94, 99½, 100½, 105½, in the ſame moment; which is pretty nearly in the proportion of 19 to 21, between the hygrometer which remains at the loweſt, and that which is at the higheſt.

61. Beſides this difference between the relative altitudes of theſe four inſtruments, I have obſerved another kind of irregularity in them, which is, that they do not always preſerve the ſame proportion to each other. Theſe variations are undoubtedly in part owing to the cauſe itſelf of their motions; that is, to the unequal diſtribution of humor even in places very near each other; but I have reaſon to aſcribe part of them to ſome defect in the inſtruments themſelves. I ſhall hereafter return to theſe cauſes, and give them a cloſer examination.

CONSIDERATIONS *on the Degree of* ACCURACY *that has been obſerved.*

62. Notwithſtanding the defects I have mentioned were rather evident, I was not diſſatisfied with this firſt trial. I never imagined that I had foreſeen every thing, and conſequently could not expect to arrive at a ſufficient degree of exactneſs without the help of experience; the irregularities therefore which appeared in the execution, did not make me deſpair of being able to perfect that inſtrument.

63. My hopes in regard to this were at firſt only grounded upon general reflections. I recollected what the barometer and thermometer had been when they firſt came out of the hands of their inventors; and obſerved that in ſome reſpects they were more

irregular than my hygrometer is at preſent. Though the firſt of theſe inſtruments was very ſimple in itſelf, yet barometers hung up in the ſame places uſed to vary three or four lines from each other. Some of the members of the French Academy have been themſelves engaged in conſidering a barometer that always kept 18 lines below the reſt, and they have formed various hypotheſes to account for this difference. The variations therefore of the barometer, though obſerved only in the ſame place, were much greater than thoſe between my hygrometers.

64. Nor was the thermometer itſelf, which is now brought to ſuch a degree of accuracy, much ſuperior at firſt to our hygroſcopes, for the purpoſe of comparative obſervations. The firſt philoſophers who treated of it knew nothing of any fixed point or determined degree in it; they knew nothing even of the effect produced by the difference of liquids. In this ſtate of uncertainty the Royal Society adopted the moſt prudent plan that could be thought of; by giving its ſanction to a thermometer to ſerve as a ſtandard for the conſtruction of thoſe which philoſophers ſhould make uſe of. After this ſome men of genius endeavoured to eſtabliſh fixed principles for the making of this inſtrument. Sir Iſaac Newton led the way, but the utility of his firſt attempts was not ſufficiently attended to. Fahrenheit and Reaumur then laboured with great care to ſettle this point, and we are much indebted to their inquiries. But Fahrenheit's principles were ſoon rejected, as being too uncertain, though his ſcale was preſerved; and Mr. de Reaumur's, though in appearance admitted for a longer continuance, were in fact ſo indeterminate,

that, without perceiving it, a deviation from 80 to 104, was made in the ſpace between the two fundamental points of his thermometer.

65. If in the ſame manner we trace the origin of all inſtruments deſigned for nice menſuration, we ſhould find that have all been defective at firſt, and gradually brought to perfection, when men of genius have thought them worthy of their attention. Thus from the firſt watch, which depended entirely upon the unequal and uncertain action of a ſpring, a ſucceſſion of attempts has produced Mr. Harriſon's valuable time-keeper; and from the firſt balances, which were either too heavy, or too light, we have attained to thoſe ſcales of Mr. Matthey * as eaſily turned as they are accurate. What however is ſtill more aſtoniſhing is, that, notwithſtanding the importance of having fixt meaſures for the dimenſions of bodies, we have not as yet uſed any in practice, but ſuch as muſt be modeled immediately from others.

It is true, that in the conſtruction of an Hygrometer, I was aſſiſted by the general notions of regularity, gathered from the conſtruction of other meaſures of the ſame kind; and of courſe my hygrometer is much forwarder in this reſpect than the thermometer, for inſtance, was in its origin. I therefore only compare the difficulties peculiar to the hy-

* An excellent mechanic, whoſe death is a loſs to a king who knows the value of men of merit. He was a native of Vale-Orbe, in the Pays de Vaud, and in the ſervice of his Sardinian Majeſty; and has written a *Treatiſe on Balances*, which ſerves as a law to all the ſcale makers in the dominions of that prince.

grometer,

grometer, to thoſe that firſt occurred in all meaſures of phyſical cauſes; and I think that as the latter have been ſurmounted, we ſhould not deſpair of conquering the former. In a word, it is certain that all our inventions only approach towards perfection by degrees, without ever attaining to it entirely; and for this very reaſon, we have a right to expect they will always be drawing nearer and nearer towards it.

Upon theſe notions chiefly, I have raiſed my hopes, either that my hygrometer will in time become more perfect, or at leaſt that it may excite new ideas, which, will at length, though perhaps by ſome other road, lead us to a true meaſure of the humor. As the hope of attaining an end, is one of the moſt powerful aſſiſtants towards really arriving at it, I flatter myſelf at leaſt that I ſhall have given birth to a reaſonable one upon this ſubject.

Firſt Views to improve the HYGROMETER.

66. The idea I entertain, that it is neceſſary a number of attentive men ſhould concur, to improve the human inventions; has induced me firſt to mention the general reaſons I had, for hoping that the hygrometer would be perfected. I ſhall now proceed to give ſome particular reaſons on which this hope is founded, and which are collected from the remarks I have already made upon my inſtrument, during the little time I have had to obſerve it.

The firſt, and one of the moſt important of theſe remarks, is, that the ivory pipe belonging to that hygrometer which is always the higheſt upon its

ſcale,

ſcale, happens at the ſame time to be the thinneſt of them all. What connexion there may be between theſe two circumſtances, muſt be determined by experience. But in the mean time it appears to me that if the fibres of the ivory are interwoven with each other; they will make ſo much the leſs reſiſtance either to the being ſeparated or brought cloſer to each other, in proportion as the bundles of theſe fibres have a leſs degree of thickneſs. Whether this remark is of conſequence, or not, we ſhall at leaſt run no riſque in making theſe ivory pipes always exactly of the ſame thickneſs. This indeed was my intention in thoſe which I have made, but unfortunately I thought I ſhould have been able to turn them upon cylinders of hard wood; and found too late, that no accuracy could be expected from this method. It was to remedy this inconvenience, that, in ſpeaking of the manner of turning this piece, I have recommended braſs cylinders (31).

67. The ſame precaution is likewiſe neceſſary to be taken, that we may be certain of giving to every pipe an equal degree of thickneſs throughout the whole of its circumference: a circumſtance no leſs eſſential than the former; ſince I have obſerved in thoſe of my hygrometers whoſe pipes have not an equal thickneſs, that they bend, more or leſs according to the degree of humor to which they are expoſed.

This is probably the principal cauſe why theſe inſtruments do not always preſerve the ſame proportions to each other (58). For the pipes not bending according to the ſame law, there muſt be an irregular

regular change in their capacity, and conſequently in the height of the mercury in the tubes. The differences of this kind which I have had occaſion to obſerve, are not indeed very conſiderable; but, however trifling the cauſe of an imperfection may be, it is ſtill uſeful to remove it; were it only to aſſiſt us in diſcovering cauſes of greater imperfections, by making their effects the more evident.

68. But to make the ivory pipes keep ſtraight, we muſt attend to a circumſtance ſtill more important; which is, that the texture of the ivory be the ſame in the whole circumference of the pipe. There is a ſenſible difference in the organization of the external, middle, and internal parts of the ſame elephant's tooth: nor is it impoſſible that, beſides this difference in the nature, and viſible arrangement of its fibres, there may be another ariſing from their degree of tenſion; ſo that ſome fibres may be more diſpoſed than others to relaxation, after the tooth has been cut to pieces. Suppoſe then that any of theſe differences ſhould exiſt in a pipe, that is, if one of its ſides ſhould be more porous, or of a weaker texture, than the other; or if its fibres ſhould be more diſpoſed to relaxation; this pipe will take a bend, either for a conſtancy, or for a time; and the hygrometers in which it is uſed, will not of courſe agree with the others. We muſt therefore endeavour to make theſe pipes with a part of the tooth that is homogeneous: that which I believe to be moſt ſo, within a certain extent, and which for that reaſon I have adviſed, is, the part which is between the center and the ſurface, and at ſome inches diſtance from the apex of the tooth (30).

69. There

69. There is another reaſon why this different organization of the different parts of the elephant's tooth makes it neceſſary to determine exactly the parts that are to be made uſe of in hygrometers. Without this precaution it might happen that the pipes, which ought to be ſimilar in every reſpect, might be made of ſubſtances that really differed in their dilatability and ſenſibility; that is, of ſubſtances which the humor might affect more or leſs ſtrongly, or more or leſs quickly. This conſideration will perhaps oblige us to determine both the ſize of the tooth, and the diſtance at which the piece ought to be cut off from its apex: for the organization may with equal probability vary in teeth of different thickneſs, and from the apex to the baſe; as it does in the breadth of the ſame tooth. I was not ſufficiently certain of the ſucceſs of my inſtrument, to take all theſe precautions when I firſt ſet about it, but at preſent I believe them to be important.

70. There is ſtill another precaution, which indeed I thought neceſſary from the beginning, but which I could not manage as I wiſhed for want of proper tools; that is, to perforate the ivory pipe exactly in the direction of its fibres. For let the channel have ever ſo ſmall a degree of obliquity with reſpect to this direction of the fibres, theſe fibres will neceſſarily be cut in different places; which weakening the pipe where it happens, neither its dilatations nor its contractions can of courſe be regular.

71. I own here are a great number of precautions; but they will not ſurprize true philoſophers. They are accuſtomed to obſerve the operations of

nature cloſely; and know that the regularity of her proceedings is connected with a foreſight whch is limited to us, by nothing but the limits of our abilities in tracing it; and conſequently, when art attempts to imitate nature, it can only ſucceed in as much as it is attentive to imitate her care.

72. I believe that the hygrometer may farther acquire a perfection of the ſame kind as that which, in conformity to an idea of my worthy friend Mr. Le Sage, I have given to the thermometer; that is, that we may make its degrees correſpond with equal differences in the humor; as I have made thoſe of the thermometer correſpond with equal degrees of heat. The way in which I think this might be done, would be to ſuſpend near one of the hygrometers, in a proper veſſel which ſhould be placed in one of the ſcales of a balance that turns very eaſily, ſome ſubſtance remarkably greedy of the humor; the augmentations or diminutions of weight in which ſubſtance, might be compared with the going of the hygrometer, firſt in the ſame, and afterwards in different degrees of heat. I hope too, that by a frequent repetition of theſe obſervations, at times when the variations of the humor are more or leſs ſudden, we ſhall at laſt ſucceed in correcting the errors that may attend them, from the loſs of its own matter the ſubſtance made uſe of may probably ſuffer by evaporation.

73. Theſe are not the only remarks I have made upon my inſtrument, but I did not care to mention any but ſuch as have appeared to me moſt certain. The others are uncertain, and require longer obſervations. I ſhall only add therefore, that it will ſtill

be neceſſary to make freſh experiments, in order to determine the length of time that the ivory pipes ought to remain in water, and how long they muſt afterwards be expoſed to the viciſſitudes of the air (28), or in general to what preparation they muſt be ſubmitted, in order to acquire a laſting degree of conſiſtence before they are made uſe of. For this purpoſe it will alſo be expedient to compare hygrometers recently made, with older ones, both to aſcertain whether they have undergone any alterations, and in what degree. I likewiſe am of opinion, that when we wiſh to fix the point of extreme humidity, we muſt be very careful not to make uſe of any ice but what is very clean, as well internally as externally; leſt any duſt ſhould ſtick to the ivory pipes, which might hinder the water from penetrating into the pores: this is what I thought of myſelf too late. I do not know whether for the ſame reaſon it would not be right to waſh theſe tubes with ſpirits of wine before we put them into the water, to remove any greaſy ſcurf they may have gathered by handling; and afterwards to repeat this at times, in order to carry off any little depoſit of various kinds, which may in courſe of time have been left upon them by the air. Moreover it will be right to inquire whether there is not a difference between the effects of the heat upon the ivory of the hygrometers, and upon the glaſs of the thermometer, ſenſible enough to be attended to, in correcting the effects of this cauſe upon the hygrometer.

74. Having already diſcovered ſo many cauſes, more or leſs probable, of the differences I obſerved

in my hygrometers, I think it reaſonable to hope that this inſtrument will receive a ſenſible degree of perfection on a ſecond trial; and that in time it will be brought to a ſufficient degree of accuracy. It is true there are ſome difficulties in the way of this: but have we not ſufficient motives for endeavouring to overcome them? The air we breathe, and that which ſurrounds us; the places we inhabit, and thoſe which ſerve either to encloſe or to preſerve ſo many different bodies intended for our ſeveral occaſions, are all of them more or leſs filled with that ſubſtance, differently modified, to which I have given the name of *humor*. It alſo produces very ſenſible effects in them; ſome of which very properly excite our curioſity, others may be turned to our advantage, and many of them eſſentially affect our health. It is therefore of great conſequence to natural philoſophy in general, and to œconomy, and medicine, in particular, that we ſhould obtain a meaſure by which we may, with ſome degree of certainty, eſtimate the local and actual qualities of this ſubſtance, and by this means foreſee its effects; which for the generality we only become acquainted with after they are produced. Theſe ſciences are not perhaps leſs concerned that we ſhould diſcover the nature itſelf of this agent, and the different manners in which it operates: the knowledge of which may enable us to avail ourſelves of reaſon in the inveſtigation of certain effects, which, without ſuch helps, might eſcape our obſervation. As theſe are the ſeveral uſes of an exact hygrometer, we may readily perceive how many new tracks ſuch an inſtrument may open to us, in our inveſtigations of nature, which however

we ſhall not owe to one man alone, but to the joint labours of ſeveral.

Account of ſome of the firſt Phænomena of the HUMOR *obſerved with the* HYGROMETER.

75. Though my firſt advances in this new track of obſervation are as yet very uncertain, yet I will not omit giving ſome account of them. They will at leaſt ſerve to give ſome idea of the going of the inſtrument, as well as of the nature of the agent by which it is governed.

The firſt obſervation I attempted of this kind was with a view to one of the objects which made me deſirous of having an hygrometer. Theſe objects are all comprized in a general ſyſtem concerning vapours, which I have given in my work upon the *Modifications of the Atmoſphere*. I ſhall therefore only mention here one particular conſequence of that ſyſtem, which it was my immediate point to verify; namely, that a certain augmentation of heat, we always perceive at every ſeaſon upon the approach of rain, is owing to a more than ordinary quantity of vapour; and that, on the contrary, it is to their diminiſhed quantity that the leſſer heat of the upper parts of the atmoſphere is in great meaſure to be aſcribed.

76. This latter conſequence was ſupported by an accidental obſervation I made in September, 1770, upon a mountain of the Faucigny, at the height of 1560 toiſes above the level of the ſea. An iron ferule, which ſerved to unite the ends of a cleft ſtick, and which had been fixed on the ſtick with a

hammer

hammer upon the plain, in fine weather, came off of itſelf upon the top of the mountain. When this happened, the thermometer, which I called common, though expoſed to the ſun, was only three degrees above 0; while on the plain it was at 18 in the ſhade. This phænomenon, joined with ſeveral others I obſerved at the ſame time, confirmed me in my opinion, that one of the reaſons why the upper parts of the atmoſphere have leſs heat than the lower, is, that they contain leſs humor.

77. With this notion, it became a very intereſting inquiry to know the different degrees of humidity habitual to the different heights of the atmoſphere. Of courſe therefore this was the firſt obſervation I thought of, as ſoon as I had added an hygrometer to the other inſtruments contained within the box of my portable barometer. I undertook therefore to aſcend Buet (the name of that high mountain) a ſecond time. My companions in this expedition were Mr. Dentan, a very intelligent young philoſopher, and my brother, who, having aſſiſted me in all undertakings of any difficulty, had been a witneſs of that fact which was the object of my preſent reſearches.

78. At our ſetting out, on the 29th of laſt Auguſt, the hygrometer was at 86 in my apartment, and the barometer at 27 p. 1 line. We were in hopes of fine weather, becauſe it is generally fair in this country when the barometer at Geneva is above 27 French inches. Soon after we ſet out, we began to perceive that the power of the ſun was greater than might have been expected for the ſeaſon. From this circumſtance I concluded that the barometer muſt fall;

fall, and in fact we found it lower at every place in our way, where we had before obſerved it in fine weather. The ſky, notwithſtanding, was ſtill clear, and continued ſo the next day, when we began to aſcend the mountain, about two o'clock in the afternoon, in order to paſs the night in the higheſt cottages, that we might have more time to gain the ſummit the next day.

79. Before we left Sixt (an abbey at the foot of the mountain), I expoſed the hygrometer in open air, and in the ſhade it ſtood at 94. The thermometer at the ſame time was at 19 in the ſhade, and at 24 in the ſun. At five o'clock we reached a place above 300 toiſes above the abbey; commanded on all ſides by mountains, and on that account called *Les Fonds* (or The Bottoms). Here we obſerved the thermometer and hygrometer. The former, when expoſed to the ſun, ſtood at $15\frac{1}{4}$, and the latter roſe to 96 in the ſhade. We obſerved them again in the ſame manner about half an hour after ſix, in a place that was pretty open, and higher by 160 toiſes than the former. The thermometer ſtood at 15, and the hygrometer at 106. It wanted but a quarter of nine, when we came to the cottages where we were to paſs the night; though they were not above 30 toiſes higher than the place we ſtopped at laſt. The higher we went, the clearer the ſky appeared; in ſo much that, notwithſtanding the uſual augmentation of humor in the air after ſunſet, when the ſky is not clouded, upon expoſing the inſtrument to the air, about $\frac{1}{4}$ after ten at night, we found the hygrometer at 123, and the thermometer at $13\frac{3}{4}$. They both fell in the night, and on our

ſetting

ſetting out the next morning, the former had got down to 109, and the latter to 12.

80. In the two laſt mentioned obſervations the hygrometer had been expoſed long enough to the open air, to conform itſelf to the degree of humor prevalent in the place; but we had not time for the obſervations I was moſt deſirous to make with accuracy. The hygrometer being uſually ſhut up in the box of my barometer, it would have been neceſſary to have left that open ſome time, in order that it ſhould adapt itſelf to the ſtate of the air, and we could allow but a very ſhort time for theſe obſervations.

81. The firſt of them was made at nine in the morning, at the height of about 1000 toiſes above the plain. The ſky appeared clear over head, but the plain was darkened with vapours. The thermometer in the ſun ſtood at 13¾, and the hygrometer roſe to 115 in the ſhade.

82. It was two in the afternoon when we reached the top of the mountain, which is always covered with an enormous maſs of ice and ſnow. We found there a very ſtrong ſouth wind, which is the warmeſt wind in our plains: beſides this, we were nearly at the hotteſt time of the day: and yet the thermometer, upon being expoſed to the ſun, ſhewed only 6. The wind, and the coldneſs of this region, obliged us to quit the ſummit in a quarter of an hour, during which the hygrometer had riſen only to 119; but we judged that it was not yet ſtationary.

83. In this ſhort time we experienced a new effect of the diminiſhed humidity of the air, which ſurprized us all three very much. We found our ſkin withered

withered and pale, ſo that both to the ſight and to the touch, it reſembled much a dry and ſhriveled bladder. Notwithſtanding this we were ſenſible of no other inconvenience but what aroſe from the wind and the cold: the action of the lungs and the functions of all the other parts of the body were perfectly free, though the barometer was only at 19 inches, 6 lines and a half.

84. We quitted the ſummit at about a quarter after two, to ſhelter ourſelves from the wind behind ſome rocks, which were nearly 50 toiſes lower. Here we ſtayed about an hour. During this time the hygrometer, expoſed to the air, but always in the ſhade, roſe by imperceptible degrees to 132$\frac{1}{4}$. It would probably have riſen higher, had not we been obliged to quit this place, where the clouds began to gather, in order to reach the cottages before night. It was indeed already too late before we thought of retiring; for we were overtaken by the night, and a thunder ſtorm, at a ſufficient diſtance from our hut to expoſe us to the greateſt danger of being loſt, notwithſtanding our guides, but for the aſſiſtance of two women, whoſe humanity deſerves the higheſt commendations. Theſe women, who lived in our cottages, being apprized of our diſtreſs by our cries, notwithſtanding the ſtorm, and the ſcarcity of wood in theſe places, came out to kindle a great fire at the foot of the rocks on which we were wandering amidſt the precipices, in total darkneſs; and ſometimes with great difficulty keeping the fire alive, ſometimes advancing towards us with fire-brands till the wind and rain extinguiſhed them, and endeavoured, with the moſt unaffected coucern, to point out to us the

path we ought to keep. At laſt, animated by the courage of theſe women, directed partly by their light and partly by their cries, we at length reached the cottage, much more affected with the humanity of theſe good people, than hurt with the dangers and fatigues we had undergone.

85. The ſtorm laſted a great part of the night, and it rained almoſt without intermiſſion. Notwithſtanding this, the hygrometer, when expoſed to the air the next morning, ſtood at 105, and the thermometer at 10. As we were uncertain how long the rain would continue, we ſet out at eight in the morning on our way down. The rain hardly ceaſed the whole morning, and was ſometimes accompanied with hail; it ſtill continued raining when we arrived at the abbey about noon, notwithſtanding the hygrometer ſtood there at 99, that is to ſay, five degrees higher than when we ſet out; but the barometer, which had fallen the two preceding days, was now beginning to riſe; the thermometer was at 14.

86. We learnt at Sixt, that, at the very time we were driven from the ſummit of the mountain by the diſagreeable coldneſs of the air, they had felt an exceſſive degree of heat, and likewiſe that the ſtorm had been very violent in the night. This ſtorm, as we found two days after at Geneva, had extended itſelf all over the plain. We found likewiſe, from the obſervations that had been made there in our abſence, that a thermometer expoſed to the north, conſequently out of the ſun, had been at $23\frac{1}{2}$, at the very time that ours, at the top of the mountain and in the ſun, had been only at 6.

87. As,

87. As, in mentioning the particular purport of the foregoing obſervations, I have not explained my ſyſtem concerning vapours, I ſhall not here ſtop to draw the conſequences that may be deduced from them in favour of this ſyſtem. Indeed, to ſay the truth, I think them too few and too imperfect to conclude any thing from them as yet. I have only related them, as I declared at firſt, to give a general notion, both of the going of my hygrometer, and of the inquiries that may be purſued with its aſſiſtance. It is with the ſame intention that I proceed to relate ſome obſervations of another kind.

88. Some accidental obſervations had made me ſuſpect that the immediate action of the ſun upon my hygrometer produced a drying, which might not be wholly occaſioned by the real ſtate of the air with reſpect to the humor, but might depend in ſome meaſure upon ſome ſingular property of the ſolar rays, which we ſee produce effects upon ſome bodies, not immediately to be accounted for by the ordinary laws of heat. This firſt remark induced me, as I have taken care to mention, always to obſerve the hygrometer in the ſhade upon the mountains of Sixt. At my return, I determined to examine more accurately whether my conjecture in this reſpect had any foundation.

89. The firſt thought that occurred to me for this purpoſe was, to obſerve two hygrometers at the ſame time, one in the ſhade and the other in the ſun, very near each other, that the ſame air might circulate freely round them. The air of the country having appeared to me more proper for this obſervation than

that of the town, I determined to obſerve at the ſame time the variations of the humor in the open air for a whole day together. There are doubtleſs many varieties in this reſpect; nor indeed ſhall I determine from this obſervation, any thing more than the ſtate of the open air during one day, and in one particular ſpot.

90. I made my obſervation the 13th of September 1772, in a garden ſituated to the weſt of our lake, and only ſeparated from it by another garden and ſome buildings. There I hung up a couple of hygrometers which I kept perfectly inſulated, one of which had no other frame but a ſcale fixt to its tube, and the other was in a frame whoſe opening at the height of the ivory tube was of a conſiderable ſize. They were four feet and a half above the ground, and at the diſtance of a foot from each other. A piece of paſteboard about 12 inches in breadth, placed at a foot's diſtance from the hygrometer which was not mounted, was intended to ſhelter it from the ſun. Each hygrometer had a thermometer cloſe to it, the ball of which was not in contact with any thing. I have proved in my work, that it is neceſſary to keep this ball inſulated, in order to obſerve the heat of the free air.

91. One of the hygrometers I made uſe of for this obſervation, was at 93, and the other at $96\frac{1}{2}$ in my room the night before. In order to correct this difference, which I ſhould ſuppoſe to be in proportion to their height, I would always add about $\frac{1}{27}$ to the height of that one which kept itſelf the loweſt, that there might be no difference between them, but ſuch as ſhould be produced by the difference

ference of the quantity of the action of the humor. The hygrometer which remained at the loweſt was the one that was always in the ſhade, and was not mounted. It was the ſame upon which I had made my obſervations in the mountains of Sixt. I ſuſpended them both in the garden I have been ſpeaking of, about 6 in the morning; the plants were covered with dew; the ſun, being juſt riſine, could not yet ſhine on the garden. As ſoon as the hygrometers were expoſed in the open air, they both fell very rapidly, but the one which was without the frame fell much faſter than the other. They both were continuing to fall, when the ſun began to ſhine in the garden. The following is an account of their progreſs, and of that of the thermometers during 19 hours. The action of the heat upon the mercury of the hygrometer is corrected upon each of them, from the obſervation of the thermometer joined to it, ſo that there only remains that of the humor.

A TABLE

A TABLE of OBSERVATIONS made on the 13th of September, on two HYGROMETERS, the one in the Shade, and the other in the Sun, each of them accompanied with a THERMOMETER.

	Hour of the Day	Therm. in the Shade.	Hygrom. in the Shade.	Hygrom. in the Sun.	Ther. in the Sun.
The Bar. at 27 Fr. inch. 1 line. The ſun did not ſhine yet on that part of the garden.	7	8	29	36½	8
The ſun has now ſhone for ¼ of an hour on the Hygr. and Therm. which are to ſtand expoſed to it.	7½	11¾	36½	66½	12
	8	12¼	43½	82	12½
	9	13	67	102	13½
	10	14½	76½	109	15½
	11	15	87½	116	16¾
	Noon	15½	96½	120½	17¼
	1	16¾	103	126	18
The vapours condenſing in the air weaken the action of the ſun.	2	16¼	103	125	17¼
Barom. 27. inch. A South wind begins to blow.	3	16¾	102½	123	17¼
The clouds riſe.	4	15¾	107	133	16
The clouds meet, and the ſunſhine withdrawn.	5	13¼	88½	106	13¼
The ſun is ſet, and the weather quite overcaſt.	6	12	64½	81	12
Barom. 26 inches 11 lines.	7	11¼	50	65	11¼
	8	11	37	50	11
	9	10¾	31	41	10¾
The clouds break, and the dew begins to appear on the plants.	10	10½	24	35	10½
	11	10	20½	26½	10
The clouds meet again.	Mid.	10½	24½	28½	10½
	1	11¼	23	27	11¼
It begins to rain.	2	11½	27	32	11½

92. The

92. The firſt circumſtance in theſe obſervations that deſerves to be noticed, is the difference in the ſinking of the two hygrometers when they were expoſed to the air, before the ſun ſhone in the garden. They both of them fell conſiderably, but one of the two 7 degrees and a half leſs than the other. One of the cauſes of this diſparity is probably in the inſtruments themſelves, and is owing to their being differently affected by the action of the humor. There is a difference of the ſame kind obſervable in the thermometers, which are likewiſe more or leſs ſenſible to the impreſſions of the heat even when the bulk of their liquid is the ſame; that is to ſay, they are acted upon more or leſs quickly by the degree of heat which ſurrounds them, according to the thickneſs, or even according to the nature of the glaſs of which the ball is made. Conſequently it is poſſible that the different thickneſs or poroſity of the ivory may have had ſome influence on the going of the hygrometer in this obſervation (66 and 69).

93. But theſe differences in the ivory pipes muſt produce a much greater difference in the ſenſibility of the hygrometers, than thoſe of the glaſs balls can produce in the thermometers; becauſe it is much more difficult for the humor to penetrate the ivory, than for the heat to get through the glaſs. So that any encreaſe of the obſtacles retards the introduction of the humor, much more than that of the heat; and conſequently the difference of ſenſibility muſt be more difficult to be prevented in the hygrometers, than it is in the thermometers.

This ſlowneſs of the humor in pervading the bodies into which it inſinuates itſelf, makes it a de-

ſirable circumſtance, that the ivory pipe of the hygrometer ſhould be the thinneſt poſſible; in order that it might be more readily affected. This I had foreſeen, before I had learnt it from experience; but I was afraid of its being attended with ſtill greater inconveniences than that it was intended to remedy; from the action of the mercury againſt pipes whoſe ſides would be thinner. However, this might be tried. In the mean time, I fancy that, for obſervations in which it is abſolutely neceſſary that the inſtrument ſhould eaſily be affected, leſſer hygrometers might be made, whoſe tubes containing a leſs quantity of mercury, would reſiſt the action of it, though with a leſs degree of thickneſs. (Perhaps it would not be impoſſible to uſe tubes made of ſome very thin quills.) I cannot yet aſcertain whether theſe little hygrometers could be graduated by themſelves, or whether they muſt be compared with thoſe of which I have given the dimenſions; this we ſhall learn from experience.

94. The difference there is between the heat and the diſcrete humor in the power of diffuſing itſelf, occaſions in another reſpect a conſiderable difference in the goings of the thermometer and hygrometer. The heat is brought into a ſtate of equilibrium much ſooner and with much greater certainty than the humor. Two thermometers accurately conſtructed and fixed near each other, in a place where the heat does not change very ſuddenly, always agree together. This is not the caſe with two hygrometers: they ſeldom agree, that is, they ſeldom preſerve the ſame conformity to each other, when there is the leaſt variation in the humor: at ſome times their difference increaſes, at others it diminiſhes; this can only

only ariſe from a difference in the cauſe that acts upon them.

95. We may form our ideas of the manner in which the inviſible humor diſtributes itſelf; from that in which all kinds of viſible vapours are diffuſed. We ſee them ſeparate, re-unite, fly off from certain places, ruſh into others, and in ſhort yield to every impreſſion of the air. The motion peculiar to their own particles, which I look upon as the cauſe of their elaſticity *, is not ſufficiently rapid, and the vapours themſelves are too thick to overcome always the contrary motion of the air. This, I believe, is what conſtitutes the chief difference between vapours, and the igneous fluid, as far as relates to the power of putting themſelves into a ſtate of equilibrium in the air, which is moving. The current of air carried towards a chimney which has fire in it, frees the room from ſmoke, and is but a very ſlight impediment to the diffuſion of the heat through it.

96. Though the inviſible vapours by reaſon of their exceſſive thinneſs are more capable of being put in equilibrium in the air than the viſible ones, they are very far from having this property in as great a degree as the heat. Which leads me to think, that part of the difference obſerved between my hygrometers, even before ſunriſe, may have been owing to the unequal diſtribution of the humor, though the two inſtruments were only at the diſtance of a foot from each other, without the interpoſition of any ſolid body.

* The ſyſtem I adopted on that point may be found in my work upon the *Modifications of the atmoſphere.*

97. I ſhall not attribute intirely to the ſame cauſe, the great difference obſerved between my hygrometers, when one was expoſed to the ſun, while the other ſtood in the ſhade. The immediate action of the ſolar rays, or of the luminous heat, produces a variety of effects, which, as I have ſaid before, do not appear to follow the ſame laws as thoſe of dark heat. And if I may be allowed to propoſe a conjecture upon this particular point, before fuller experiments have been made, it ſhould ſeem, that the immediate action of the ſolar rays muſt occaſion a greater evaporation than what is produced by dark heat, even when they hold the thermometer at the ſame height. But let the cauſe be as it will, we ſee by this experiment, that in a ſection of air about a foot wide, through which the ſolar rays did not immediately paſs, the action of the humor upon the hygrometer was 23 degrees greater than in the place round about; though that of the heat upon the thermometers was only a degree and a quarter leſs; which leads us to conceive how many apparently ſmall cauſes may contribute to produce ſenſible differences in the diſtribution of the diſcrete humor.

98. Another uſe to be made of theſe obſervations is, to compare them with thoſe that I have made in the mountains of Sixt; in order to form a better judgment of the proportion between the different degrees of humidity, in the ſuperior and inferior parts of the atmoſphere. My hygrometer, held in the ſhade upon the ſummit of Buet, roſe to 132½, and was not yet ſtationary. This is pretty nearly the greateſt degree of dryneſs obſerved in the hygrometer expoſed to the ſun in the garden; while the

the one that remained in the ſhade, the ſame upon which the obſervation at the mountain had been made, was not in fact higher than 103, though marked in the table of obſervations at 107 (91).

99. But the difference between the obſervations made upon the mountain of Sixt, and thoſe I am ſpeaking of, was ſtill greater by much after ſun-ſet. The 30th of Auguſt, at a quarter after ten at night, I obſerved the hygrometer without ſide the cottage upon the mountain, and found it at 123 (79); and on the 13th of September following, in the plain, it was not higher than 31 at 9, and 24 at 10 o'clock. The wind was ſouth, and the height of the barometer upon the plain, pretty much the ſame during both the obſervations.

100. It is true that, notwithſtanding the ſimilarity of theſe circumſtances, theſe obſervations cannot be directly compared, on account of the diſagreement in ſome other circumſtances. In the firſt place, the difference of fourteen days at this ſeaſon of the year may have produced a ſenſible change in the ſtate of the air. There was already, for inſtance, a conſiderable difference in the degrees of the thermometer; it was at 13 and $\frac{3}{4}$ when the obſervation was made on the mountain, and no higher than at 10 on the plain. Beſides, at this time of night, there would always be an eſſential difference between the upper and lower parts of the atmoſphere, even though in the day time they ſhould have the ſame degree of humidity: for the vapours being condenſed after ſun-ſet, and thus producing a kind of dew, they muſt neceſſarily deſcend, and from this very cauſe be more abundant in the low grounds than

on the higher ones. I ſhall add, that though my hygrometer was expoſed to the open air on the mountain, as it was in the plain, yet it was not ſo much inſulated there, being tied to the box of my portable barometer. The difference obſerved, however, is ſo conſiderable, that, notwithſtanding the concurrence of all theſe particular cauſes, I cannot but aſcribe it in ſome meaſure to that general one which I have ſuſpected, namely, that there is comparatively a leſs degree of humidity in the upper than in the lower parts of the atmoſphere.

101. The obſervation of the 13th of September ſeems likewiſe to throw ſome light upon the phænomena of dew. We know that when the ſky is cloudy, there is little or no dew, and it has likewiſe been obſerved from this very circumſtance, that the air is not ſo much cooled after ſun-ſet. The cauſe of theſe differences appears to me to be, that when there are no clouds in the air at ſun-ſet, or when they are diſperſed, the heat of the inferior air, and that which riſes from the earth, diſſipates itſelf into the ſuperior regions, and then the vapours which are diſperſed throughout the air condenſe and fall down again in dew; but when the clouds are continued, and thus ſeparate the inferior from the ſuperior air, they prevent this diſſipation of the heat, and the vapours remain ſuſpended. And if the ſky grows cloudy ſome hours after the ſetting of the ſun, and after the heat has ſenſibly diminiſhed in the inferior air, it encreaſes again in it; becauſe the heat, which continues to riſe out of the earth, is accumulated in the inferior air. This appears in the obſervation I am ſpeaking of. The clouds having been ſepa-

rated for a while, at 10 o'clock there was ſome dew, and the hygrometer fell ſenſibly till eleven : but afterwards the clouds cloſing again, the heat encreaſed, and the humidity evidently diminiſhed.

102. I take it for granted here, that the moſt common and moſt plentiful dew proceeds from the air, and not from the earth, as ſome philoſophers have imagined. I ſhould produce the proofs I have collected of this fact from a multitude of experiments, if it had not been done in an excellent paper, written by Profeſſor le Roi, *On the elevation and ſuſpenſion of water in the air* *. Theſe phænomena of the dew become very intereſting examined with the help of the hygrometer, and joined to obſervations of the degrees of ſaturation of the air with reſpect to water, which have been ſo ingeniouſly imagined, and begun by the author of this memoir. If this part of natural philoſophy is ever cleared up, as I hope it will be, we ſhall be much indebted for it to the ſagacity of this true philoſopher.

103. I ſhall only mention one more obſervation I have endeavoured to make with my hygrometer, which ought not to be omitted, as it is connected with the principles upon which the inſtrument is conſtructed. It has likewiſe a reference to medicine, in as much as one of the objects of that ſcience, in its inquiries to preſerve our health, is to determine the effects of water at different degrees of heat upon our organs. Ivory being an animal ſubſtance, the effects produced upon it by water at different degrees

* Mem. de l'Ac. des Sc. de Paris, for the year 1751.

of

of heat, may aſſiſt us in diſcovering thoſe which are produced upon our bodies from the ſame cauſe.

104. The point o of my hygrometer, as I have before obſerved (44), is that of the extreme humidity produced by melting ice. It was therefore of ſome importance to know what difference there would be in this point, when the hygrometer ſhould be plunged into warmer water. This I endeavoured to find out; and the following is the reſult of my firſt inquiries.

105. The moment I took one of my hygrometers out of melting ice, I plunged it into water at the heat of 45 degrees of the thermometer that I have called common. It fell ſuddenly four of its degrees below the thread which marked its height in the melting ice, but immediately roſe again, and in four minutes reached 8 degrees and a half above the ſame thread. Deducting $22\frac{1}{2}$ from the height for the dilatation of the mercury (48), there will remain 14. Conſequently the water warmed at 45 degrees of the common thermometer, really made the hygrometer ſink 14 degrees below o.

106. Half an hour after this, the water being at 38 degrees, I found the hygrometer no higher than $6\frac{1}{2}$, that is to ſay, $6\frac{1}{2} - \frac{38}{2} = -12\frac{1}{2}$. Conſequently the true point of humidity indicated by the hygrometer was $12\frac{1}{2}$ below o. Laſtly, the heat of the water being reduced to 28 degrees the hygrometer was at $3 - \frac{28}{2} = -11$. I was then obliged to put an end to the experiment, which I have not been able to take up again ſince, for want of leiſure. But

what has already been obſerved is ſufficient to ſhew us that the warmer the water is, the more it dilates the ivory (though we ſaw that the mercury roſe in the hygrometer after having ſunk for a moment). From hence, I fancy, may be drawn this general conſequence, already indeed foreſeen, namely, that in *an equal acting quantity*, the warmer the humor is, the more it ſeparates the particles of thoſe bodies which it pervades.

107. I ſay, in an *equal acting quantity*; and this is one of the objects which will probably furniſh us with a variety of moſt uſeful knowledge, at the ſame time that it is moſt likely to give the greateſt exerciſe to the genius and attention of natural philoſophers. The forementioned experiment proves, that the warmer the water is, the more it dilatates the ivory pipe of the hygrometer, and the ſame thing I make no doubt happens with the diſcrete humor.

On the other hand, the evaporation being certainly greater in ſummer than in winter, there muſt of courſe be more vapours in the air in the firſt of theſe ſeaſons than in the latter. Theſe then, as it appears, are the two circumſtances moſt likely to make the hygrometer fall in ſummer; a greater degree of humor in the air, and an encreaſe of heat. And yet I have already experienced that the mean height of the hygrometer is greater in ſummer than in the other ſeaſons. I found my firſt hygrometer, which was made in winter, too ſhort in the ſummer; but it would be of a ſufficient length now that we are in autumn. The mean height of the four new ones is already (the beginning of Novem-

ber)

ber) 17 degrees leſs than it was in the months of Auguſt and September.

108. I hope this paradox will be explained, and that the principles which may clear it up will draw uſeful conſequences along with them. Thoſe philoſophers who look upon evaporation as a diſſolution of water by air in the manner of menſtrua, that is, by affinity, will eaſily apply their principle to the ſolution of part of theſe phænomena. The diſſolution is greater when the menſtruum is warmer, and conſequently the air muſt keep a greater quantity of water in diſſolution, and ſuffer a leſs part of it to be precipitated, in ſummer than in winter. I cannot but allow that this ſyſtem is extremely ſpecious, and that many phænomena are very happily explained by means of it. This is what Mr. le Roy has ſhewn us in the memoir I have already quoted; in which, without contending that air really acts as a menſtruum with reſpect to water, he demonſtrates, by a parallel very well kept up, that all the chemical expreſſions concerning diſſolutions may with propriety be applied to deſcribe the ſeveral phænomena he examines, relative to the elevation and ſuſpenſion of water in air, as well as to its precipitation under different forms.

109. If it was not too common a practice, to conclude things from words, I ſhould in fact think theſe chemical expreſſions very conveniently adapted to explain a number of theſe phænomena. But I have rejected them here, on account of this conſideration; that when I took in a greater number of phænomena, I found them no longer accurate, any more than the general idea of the diſſolution

tion of water by air. I have given the reaſons for this in my work, upon the modification of the atmoſphere; and ſhall only repeat here, that theſe modifications of the humor appear to me almoſt intirely to be produced by the igneous fluid; and that if the air has any ſhare in them, it is only as being an elaſtic fluid. The particles of theſe fluids, each according to its degree of power, ſtrike, ſeparate, and draw along with them thoſe of the humor, and communicate to them the elaſticity they poſſeſs; in the ſame manner as they do to the particles of all volatile, and likewiſe of all fixt ſubſtances which they corrode and decompoſe.

110. This ſyſtem will not only furniſh a ſolution of the paradox which engages our attention, but will, I believe, carry us much farther. The heat of the ſummer keeps the humor in very great agitation; and though there is more of the humor at this ſeaſon than in winter, yet this heat will not allow it to continue either as long a time, or in as great a quantity, upon the bodies or in their pores. That is the reaſon why the hygrometer falls leſs. But we ſee at the ſame time, that the portion of the humor which does ſojourn, and which I call the active part, has more power to dilatate the bodies, from the greater degree of motion impreſſed upon it by a greater heat. Conſequently the dilatation of the bodies, from this cauſe, will be in a compound ratio of the quantity of humor, and of its active force, or of the heat. And if, for inſtance, we compare any ſummer's day, in which the hygrometer in open air is at the ſame degree as on any winter's day, the air on the ſummer's day will contain more humor than on the

winter's day; but there will be leſs of it will act upon the hygrometer; and yet as the active part will have more ſtrength, the effect upon the whole will be the ſame. This is what appears to me, but I can not now enlarge any farther on this ſyſtem. I have ſaid enough to ſhew that the ſubject is very expenſive, and deſerves an attentive examination.

XXXIX. *Of*

Fig: II.

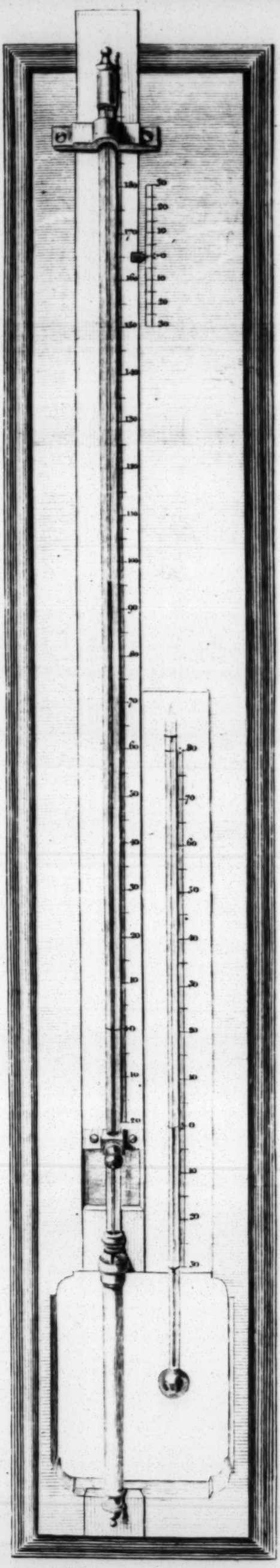


Fig: I.



Hygrometer.

J. Basire sculp.

XXXIX. *Of the electric Property of the Torpedo. In a Letter from* John Walſh, *Eſq; F. R. S. to* Benjamin Franklin, *Eſq; LL. D., F. R. S., Ac. R.* Par. *Soc. Ext.*, &c.

Cheſterfield-Street, July 1, 1773.

DEAR SIR,

Read July 1, 1773. I Am concerned that other engagements have prevented me from giving to the Royal Society, before their receſs, a complete account of my experiments on the electricity of the Torpedo; a ſubject not only curious in itſelf, but opening a large field for intereſting inquiry, both to the electrician in his walk of phyſics, and to all who conſider, particularly or generally, the animal œconomy.

To ſupply the deficiency in the beſt manner I am now able, I will requeſt the favour of you to lay before the Society my letter from la Rochelle, of the 12th July 1772, and ſuch part of the letter I afterwards wrote from Paris, as relates to this ſubject. Looſe and imperfect as theſe informations are, for they were never intended for the public eye, they are ſtill the moſt authentic, and ſo far the moſt

ſatisfactory I can at preſent offer, ſince the notes I made of the experiments themſelves remain nearly, I am ſorry to ſay it, in that crude and bulky ſtate in which you had the trouble to read them.

Letter from Mr. WALSH to Dr. FRANKLIN, dated la Rochelle, 12th July 1772.

" It is with particular ſatisfaction I make to you " my firſt communication, that the effect of the " Torpedo appears to be abſolutely electrical; by " forming its circuit through the ſame conductors " with electricity, for inſtance metals, and water; " and by being intercepted by the ſame non-con- " ductors, for inſtance glaſs, and ſealing-wax. I " will not at preſent trouble you with the detail of " our experiments, eſpecially as we are daily ad- " vancing in them; but only obſerve, that we have " diſcovered the back and breaſt of the animal to be " in different ſtates of electricity: I mean in par- " ticular the upper and lower ſurfaces of thoſe two " aſſemblages of pliant cylinders, of which you " have ſeen engravings in Lorenzini *. By the " knowledge of this circumſtance we have been " able to direct his ſhocks, though they were very " ſmall, through a circuit of four perſons, all feel- " ing them; likewiſe through a conſiderable length " of wire held by two inſulated perſons, one touching " his lower ſurface, and the other his upper. When

* Obſervazioni intorno alle Torpedini di Stef. Lorenzini 1678.

Redi appears to be the firſt who remarked theſe ſingular parts of the Torpedo in 1666. Franc. Redi, Exper. Nat.

" the

" the wire was exchanged for glaſs, or ſealing-wax,
" no effect could be obtained: but as ſoon as it was
" reſumed, the two perſons became liable to the
" ſhock. Theſe experiments have been varied many
" ways, and repeated times without number, and
" they all determined the choice of conductors
" to be the ſame in the Torpedo as in the Ley-
" den Phial. The ſenſations likewiſe, occaſioned
" by the one and the other in the human frame, are
" preciſely ſimilar. Not only the ſhock, but the
" numbing ſenſation which the animal ſometimes
" diſpenſes, expreſſed in French by the words *en-*
" *gourdiſſement* and *fourmillement*, may be exactly imi-
" tated with the Phial, by means of Lane's Electro-
" meter; the regulating rod of which, to produce the
" latter effect, muſt be brought almoſt into contact
" with the prime conductor which joins the Phial.
" We have not yet perceived any ſpark to accompany
" the ſhock, nor the pith balls to be ever affected.
" Indeed all our trials have been on very feeble
" ſubjects, whoſe ſhock was ſeldom ſenſible beyond
" the touching finger: I remember but one, of at
" leaſt two hundred, that I myſelf muſt have re-
" ceived, to have extended above the elbow. Per-
" haps the Iſle of Ké, which we are about to viſit,
" may furniſh us with Torpedos freſher taken and of
" more vigour, by which a farther inſight into theſe
" matters may be had. Our experiments have been
" chiefly in the air, where the animal was more
" open to our examination than in water. It is a
" ſingularity that the Torpedo, when inſulated,
" ſhould be able to give to us, inſulated likewiſe,
" forty or fifty ſucceſſive ſhocks from nearly the

" ſame part; and theſe with little, if any diminu-
" tion in their force: indeed they were all very
" minute. Each effort in the animal to give the
" ſhock is conveniently accompanied with a de-
" preſſion of his eyes, by which even his attempts
" to give it to non-conductors can be obſerved.
" The animal, with reſpect to the reſt of his body,
" is in a great degree motionleſs, but not wholly ſo.
" You will pleaſe to acquaint Dr. Bancroft, of our
" having thus verified his ſuſpicion concerning the
" Torpedo *, and make any other communication
" of this matter you may judge proper. Here I
" ſhall be glad to excite, as far as I am able, both
" electricians and naturaliſts, to puſh their inquiries
" concerning this extraordinary animal, whilſt the
" ſummer affords them the opportunity."

Extracts of a Letter from Mr. WALSH to Dr. FRANKLIN, dated Paris, 27th Auguſt, 1772.

——— " I ſpent a complete week in my ex-
" periments at the Iſle of Ré, and had there every
" convenience for proſecuting them to their extent,
" except that I was reſtrained by the jealouſy of
" the government from making them where the
" animal was caught. At my return to la Ro-
" chelle, I communicated to the members of the
" Academy of that place, and to many of the prin-
" cipal inhabitants, all that I had obſerved con-
" cerning the Torpedo, in the intention of ſtirring

* Bancroft's Natural Hiſtory of Guiana, p. 194.

" up

" up a ſpirit of inquiry, both as to its electricity and
" general œconomy.

——— " The vigour of the freſh taken Tor-
" pedos at the Iſle of Ré, was not able to force the
" torpedinal fluid acroſs the minuteſt tract of air;
" not from one link of a ſmall chain, ſuſpended
" freely, to another; not through an almoſt in-
" viſible ſeparation, made by the edge of a penknife,
" in a ſlip of tinfoil paſted on ſealing-wax. The
" ſpark therefore (of courſe the attendant ſnapping
" noiſe) was denied to all our attempts to diſcover
" it, not only in day-light, but in complete dark-
" neſs. I obſerved to you, in my laſt, the ſingu-
" larity of the Torpedo being able, when inſulated,
" to give to an inſulated perſon a great number of
" ſucceſſive ſhocks: in this ſituation I have taken
" no leſs than fifty from him in the ſpace of a
" minute and an half. All our experiments con-
" firmed, that his electricity was condenſed, in
" the inſtant of its exploſion, by a ſudden energy
" of the animal; and as there was no gradual
" accumulation, nor retention of it, as in the caſe
" of charged glaſs, it is not at all ſurprizing that
" no ſigns of attraction or repulſion were perceived
" in the pith balls. In ſhort, the effect of the Tor-
" pedo appears to ariſe from a compreſſed elaſtic
" fluid, reſtoring itſelf to its equilibrium in the ſame
" way, and by the ſame mediums, as the elaſtic fluid
" compreſſed in charged glaſs. The ſkin of the
" animal, bad conductor as it is, ſeems to be a
" better conductor of his electricity, than the
" thineſt plate of elaſtic air. Notwithſtanding the
" weak ſpring of the torpedinal electricity, I was
" able

" able, in the public exhibitions of my experiments " at la Rochelle, to convey it through a circuit, " formed from one ſurface of the animal to the " other, by two long braſs wires, and four perſons, " which number, at times, was increaſed even to " eight. The ſeveral perſons were made to com- " municate with each other, and the two outermoſt " with the wires, by means of water contained in " baſins, properly diſpoſed between them for the " purpoſe; each perſon dipping his hands in the " neareſt baſins, connectively with his neigbour on " either ſide.——

" The effect produced by the Torpedo, when " in air, appeared, on many repeated experiments, " to be about four times as ſtrong as when in " water."

A clear and ſuccinct narrative of what paſſed at one of the public exhibitions, alluded to in the laſt letter, appeared in the French Gazette of the 30th October 1772. As it came from a very reſpectable quarter, not leſs ſo from the private character of the gentleman, than from the public offices he held, I muſt deſire leave of the Society to avail myſelf of ſuch a teſtimony to the facts I have advanced, by giving a tranſlation of that narrative.

Extract of a Letter from the Sieur SEIGNETTE, Mayor of la Rochelle, and ſecond perpetual Secretary of the Academy of that City, to the publiſher of the French Gazette.

" In the Gazette of the 14th Auguſt, you men- " tioned the diſcovery made by Mr. Walſh, Mem-

" ber

" ber of the parliament of England, and of the
" Royal Society of London. The experiment, of
" which I am going to give you an account, was
" made in the preſence of the Academy of this
" city. A live Torpedo was placed on a table.
" Round another table ſtood five perſons inſulated.
" Two braſs wires, each thirteen feet long, were
" ſuſpended to the ceiling by ſilken ſtrings. One
" of theſe wires reſted by one end on the wet
" napkin on which the fiſh lay; the other end
" was immerſed in a baſin full of water placed
" on the ſecond table, on which ſtood four other
" baſins likewiſe full of water. The firſt perſon
" put a finger of one hand in the baſin in which
" the wire was immerſed, and a finger of the
" other hand in the ſecond baſin. The ſecond
" perſon put a finger of one hand in this laſt
" baſin, and a finger of the other hand in the
" third; and ſo on ſucceſſively, till the five per-
" ſons communicated with one another by the
" water in the baſins. In the laſt baſin one end
" of the ſecond wire was immerſed; and with
" the other end Mr. Walſh touched the back of
" the Torpedo, when the five perſons felt a com-
" motion which differed in nothing from that of
" the Leyden experiment, except in the degree of
" force. Mr. Walſh, who was not in the circle
" of conduction, received no ſhock. This expe-
" riment was repeated ſeveral times, even with
" eight perſons; and always with the ſame ſuc-
" ceſs. The action of the Torpedo is commu-
" nicated by the ſame mediums as that of the
" electric fluid. The bodies which intercept the

" action of the one, intercept likewiſe the action " of the other. The effects produced by the " Torpedo reſemble in every reſpect a weak elec- " tricity."

This exhibition of the electric powers of the Torpedo, before the Academy of La Rochelle, was at a meeting, held for the purpoſe in my apartments, on the 22d July 1772, and ſtands regiſtered in the journals of the Academy.

The effect of the animal was, in theſe experiments, tranſmitted through as great an extent and variety of conductors as almoſt at any time we had been able to obtain it, and the experiments included, nearly, all the points, in which its analogy with the effect of the Leyden Phial had been obſerved. Theſe points were ſtated to the gentlemen preſent, as were the circumſtances in which the two effects appeared to vary. It was likewiſe repreſented to them, That our experiments had been almoſt wholly with the animal in air: That its action in water was a capital deſideratum: That indeed all as yet done was little more than opening the door to inquiry: That much remained to be examined by the Electrician as well as by the Anatomiſt: That as artificial electricity had thrown light on the natural operation of the Torpedo, this might in return, if well conſidered, throw light on artificial electricity, particularly in thoſe reſpects in which they now ſeemed to differ: That for me, I was about to take leave of the animal, as nature had denied it to the Britiſh ſeas; and that the proſecution of theſe reſearches

reſted in a particular manner with them, whoſe ſhores abounded with it.

The Torpedo, on this occaſion, diſpenſed only the diſtinct, inſtantaneous ſtroke, ſo well known by the name of the electric ſhock. That protracted but lighter ſenſation, that Torpor or Numbneſs which he at times induces, and from which he takes his name, was not then experienced from the animal; but it was imitated with artificial electricity, and ſhewn to be producible by a quick conſecution of minute ſhocks. This, in the Torpedo, may perhaps be effected by the ſucceſſive diſcharge of his numerous cylinders, in the nature of a running fire of muſketry: the ſtrong ſingle ſhock may be his general volley. In the continued effect, as well as in the inſtantaneous, his eyes, uſually prominent, are withdrawn into their ſockets.

The ſame experiments, performed with the ſame Torpedos, were on the two ſucceeding days repeated before numerous companies of the principal inhabitants of La Rochelle. Beſides the pleaſure of gratifying the curioſity of ſuch as entertained any on the ſubject, and the deſire I had to excite a proſecution of the inquiry, I certainly wiſhed to give all poſſible notoriety to facts, which might otherwiſe be deemed improbable, perhaps by ſome of the firſt rank in ſcience. Great authorities had given a ſanction to other ſolutions of the phœnomena of the Torpedo; and even the Electrician might not readily liſten to aſſertions, which ſeemed, in ſome reſpects, to combat the general principles of electricity. I had reaſon to

make ſuch concluſions from different converſations I had held on the ſubject with eminent perſons both at London and Paris. It is but juſtice to ſay, that of all in that claſs you gave me the greateſt encouragement to look for ſucceſs in this reſearch, and even aſſiſted me in forming hypotheſes, how the Torpedo, ſuppoſed to be endued with electric properties, might uſe them in ſo conducting an element as water.

After generally recommending to others an examination of the electric powers of theſe animals when acting in water, I determined, before I took my final leave of them, to make ſome farther experiments myſelf with that particular view; ſince, notwithſtanding the familiarity in which we may be ſaid to have lived with them for near a month, we had never detected them in the immediate exerciſe of their electric faculties againſt other fiſh, confined with them in the ſame water, either in the circumſtance of attacking their prey, or defending themſelves from annoyance: and yet that they poſſeſſed ſuch a power, and exerciſed it in a ſtate of liberty, could not be doubted.

A large Torpedo, very liberal of his ſhocks, being held with both hands by his electric organs above and below, was briſkly plunged into water to the depth of a foot, and inſtantly raiſed an equal height into air; and was thus continually plunged and raiſed, as quick as poſſible, for the ſpace of a minute. In the inſtant his lower ſurface touched the water in his deſcent, he always gave a violent ſhock, and another ſtill more violent in the inſtant of quitting the water in his aſcent;

aſcent; both which ſhocks, but particularly the laſt, were accompanied with a writhing in his body, as if meant to force an eſcape: Beſides theſe two ſhocks from the ſurface of the water, which may yet be conſidered as delivered in the air, he conſtantly gave at leaſt two, when wholly in the air, and conſtantly one and ſometimes two, when wholly in the water. The ſhocks in water appeared, as far as ſenſation could decide, not to have near a fourth of the force of thoſe at the ſurface of the water, nor much more than a fourth of thoſe intirely in air.

The ſhocks received in a certain time were not, on this occaſion, counted by a watch, as they had been on a former, when fifty were delivered, in a minute and a half, by the animal in an inſulated and unagitated ſtate: But from the quickneſs, with which the immerſions were made, it may be preſumed there were full twenty of theſe in a minute; from whence the number of ſhocks, in that time, muſt have amounted to above an hundred. This experiment, therefore, while it diſcovered the comparative force between a ſhock in water and one in air, and between a ſhock delivered with greater exertion on the part of the animal and one with leſs, ſeemed to determine, that the charge of his organs with electricity was effected in an inſtant, as well as the diſcharge.

The Torpedo was then put into a flat baſket, open at top, but ſecured by a net with wide meſhes, and, in this confinement, was let down into the water, about a foot below the ſurface; being

being there touched, through the meſhes, with only a ſingle finger, on one of his electric organs, while the other hand was held, at a diſtance, in the water, he gave ſhocks, which were diſtinctly felt in both hands.

The circuit for the paſſage of the effect being contracted to the finger and thumb of one hand, applied above and below to a ſingle organ, produced a ſhock, to our ſenſation, of twice the force of that in the larger circuit by the arms.

The Torpedo, ſtill confined in the baſket, being raiſed to within three inches of the ſurface of the water, was there touched with a ſhort iron bolt, which was held, half above and half in the water, by one hand, while the other hand was dipped, as before, at a diſtance in the water; and ſtrong ſhocks, felt in both hands, were thus obtained through the iron.

A wet hempen cord being faſtened to the iron bolt, was held in the hand above water, while the bolt touched the Torpedo; and ſhocks were obtained through both thoſe ſubſtances.

A leſs powerful Torpedo, ſuſpended in a ſmall net, being frequently dipped into water and raiſed again, gave, from the ſurface of the water, ſlight ſhocks through the net to the perſon holding it.

Theſe experiments in water manifeſted, That bodies, immerſed in that element, might be affected by immediate contact with the Torpedo; That the ſhorter the circuit in which the electricity

tricity moved, the greater would be the effect; And that the ſhock was communicable, from the animal in water, to perſons in air, through ſome ſubſtances.

How far harpoons and nets, conſiſting of wood and hemp, could in like circumſtances, as it has been frequently aſſerted, convey the effect, was not ſo particularly tried as to enable us to confirm it. I mention the omiſſion in the hope that ſome one may be induced to determine the point by expreſs trial.

We convinced ourſelves, on former occaſions, that the accurate Kæmpfer *, who ſo well deſcribes the effect of the Torpedo, and happily compares it with lightning, was deceived in the circumſtance, that it could be avoided by holding in the breath, which we found no more to prevent the ſhock of the Torpedo, when he was diſpoſed to give it, than it would prevent the ſhock of the Leyden Phial.

Several perſons, forming as many diſtinct circuits, can be affected by one ſtroke of the animal, as well as when joined in a ſingle circuit. For inſtance, four perſons, touching ſeparately his upper and lower ſurfaces, were all affected; two perſons likewiſe, after the electricity had paſſed through a wire into a baſin of water, tranſmitted it from thence, in two diſtinct channels, as their ſenſation convinced them, into another baſin of water, from whence it was conducted, probably in an united ſtate, by a ſingle wire. How-

* Kæmpf. Amœn. Exot. 1712, p. 514.

much

much further the effect might be thus divided and ſubdivided into different channels, was not determined; but it was found to be proportionably weakened by multiplying theſe circuits, as it had been by extending the ſingle circuit.

Something may be expected to be ſaid of the parts of the animal immediately concerned in producing the electrical effect. The engraving, which accompanies this letter, while it ſhews the general figure of the Torpedo, gives an internal view of his electric organs. The Society will, beſides, have a full anatomical deſcription of theſe parts from the ingenious Mr. John Hunter, in a paper he has expreſsly written on the ſubject at my requeſt. It would therefore be ſuperfluous for me to ſay any thing either in regard to their ſituation or ſtructure.

I have to obſerve, however, That in theſe double organs reſides the electricity of the Torpedo, the reſt of his body appearing to be no otherwiſe concerned in his electrical effect than as conducting it: That they are ſubject to the will of the animal; but whether, like other double parts ſo controlable, they are exerciſed, at times, ſingly as well as in concert, is difficult to be aſcertained by experiment: That their upper and under ſurfaces are capable, from a ſtate of equilibrium with reſpect to electricity, of being inſtantly thrown, by a meer energy, into an oppoſition of a *plus* and *minus* ſtate, like that of the charged Phial: That, when they are thus charged, the upper ſurfaces of the two are in the ſame ſtate of electricity; as are the under

ſurfaces of the two, though in a contrary to that of the upper; for no ſhock can be obtained by an inſulated perſon touching both organs above, or both below: And that the production of the effect depends ſolely on an intercourſe being made between the oppoſite ſurfaces of the organs, whether taken ſingly or jointly.

All the parts bordering on the organs act, more or leſs, as conductors, either through their ſubſtance or by their ſuperficies. While an inſulated perſon, placing two fingers on the ſame ſurface of one or both organs, cannot be affected; if he removes one of his fingers to any ſuch contiguous part, he will be liable to a ſhock: but this ſhock will not be near, perhaps not half, ſo violent, as one taken immediately between the oppoſite ſurfaces of the organ; which ſhews the conduction to be very imperfect.

The parts, which conduct the beſt, are the two great lateral fins bounding the organs outwardly, and the ſpace lying between the two organs inwardly. All below the double tranſverſe cartilages ſcarcely conduct at all, unleſs when the fiſh is juſt taken out of water and is ſtill wet, the mucus, with which he is lubricated, ſhewing itſelf, as it dries, to be of an inſulating nature.

The organs themſelves, when uncharged, appeared to be, not interiorly we might ſuppoſe, but rather exteriorly, conductors of a ſhock. An inſulated perſon touching two Torpedos, lying near one another on a damp table, with fingers placed, one on the organ of one fiſh, and another

on the organ of the other, was ſenſible of ſhocks, ſometimes delivered by one fiſh, and ſometimes by the other, as might be diſcovered by the reſpective winking of their eyes. That the organs, uncharged, ſerved ſome way or other as conductors, was confirmed with artificial electricity, in paſſing ſhocks by them; and in taking ſparks from them, when electrified.

The electric effect was never perceived by us to be attended with any motion or alteration in the organs themſelves, but was frequently accompanied with a little tranſient agitation along the cartilages which ſurround both organs: this is not diſcernible in the plump and turgid ſtate of the animal, while he is freſh and vigorous; but as his force decays, from the relaxation of his muſcles, his cartilages appear through the ſkin, and then the ſlight action along them is diſcovered.

May we not from all theſe premiſes conclude, that the effect of the Torpedo proceeds from a modification of the electric fluid? The Torpedo reſembles the charged Phial in that characteriſtic point of a reciprocation between its two ſurfaces. Their effects are tranſmitted by the ſame mediums; than which there is not perhaps a ſurer criterion to determine the identity of ſubtile matter: They, beſides, occaſion the ſame impreſſion on our nerves. Like effects have like cauſes. But it may be objected, that the effects of the Torpedo, and of the charged Phial, are not ſimilar in all their circumſtances; that the charged Phial occaſions attractive or repulſive diſpoſitions

in

in neighbouring bodies; and that its diſcharge is obtained through a portion of air, and is accompanied with light and ſound; nothing of which occurs with reſpect to the Torpedo.

The inaction of the electricity of the animal in theſe particulars, whilſt its elaſtic force is ſo great as to tranſmit the effect through an extenſive circuit and in its courſe to communicate a ſhock, may be a new phænomenon, but is no ways repugnant to the laws of electricity; for here too, the operations of the animal may be imitated by art.

The ſame quantity of electric matter, according as it is uſed in a denſe or rare ſtate, will produce the different conſequences. For example, a ſmall Phial, whoſe coated ſurface meaſures only ſix ſquare inches, will, on being highly charged, contain a denſe electricity capable of forcing a paſſage through an inch of air, and afford the phænomena of light, ſound, attraction, and repulſion. But if the quantity condenſed in this Phial, be made rare by communicating it to three large connected jars, whoſe coated ſurfaces ſhall form together an area 400 times larger than that of the Phial (I inſtance theſe jars becauſe they are ſuch as I uſe); it will, thus dilated, yield all the negative phænomena, if I may ſo call them, of the Torpedo; it will not now paſs the hundredth part of that inch of air, which in its condenſed ſtate it ſprung through with eaſe; it will now refuſe the minute interſection in the ſtrip of tinfoil; the ſpark and its attendant ſound, even the attraction or repulſion of light bodies, will now

be wanting; nor will a point brought however near, if not in contact, be able to draw off the charge: and yet, with this diminiſhed elaſticity, the electric matter will, to effect its equilibrium, inſtantly run through a conſiderable circuit of different conductors, perfectly continuous, and make us ſenſible of an impulſe in its paſſage.

Let me here remark, that the ſagacity of Mr. Cavendiſh in deviſing and his addreſs in executing electrical experiments, led him the firſt to experience with artificial electricity, that a ſhock could be received from a charge which was unable to force a paſſage through the leaſt ſpace of air.

But, after the diſcovery that a large area of rare electricity would imitate the effect of the Torpedo, it may be inquired, where is this large area to be found in the animal? We here approach to that veil of nature, which man cannot remove. This, however, we know, that from infinite diviſion of parts infinite ſurface may ariſe, and even our groſs optics tell us, that thoſe ſingular organs, ſo often mentioned, conſiſt like our electric batteries of many veſſels, call them cylinders or hexagonal priſms, whoſe ſuperficies taken together furniſh a conſiderable area.

I rejoice in addreſſing theſe communications to You. He, who predicted and ſhewed that electricity wings the formidable bolt of the atmoſphere, will hear with attention, that in the deep it ſpeeds an humbler bolt, ſilent and inviſible: He, who analyſed the electrified Phial, will hear with

pleaſure that its laws prevail in animate Phials: He, who by Reaſon became an electrician, will hear with reverence of an inſtinctive electrician, gifted in his birth with a wonderful apparatus, and with the ſkill to uſe it.

However I may reſpect your talents as an electrician, it is certainly for knowledge of more general import, that I am impreſſed with that high eſteem, with which I remain,

Dear Sir,

Your affectionate

and obedient ſervant,

John Walſh.

EXPLANATION of the PLATE

OF THE

Male and Female Torpedo, or Electric Ray.

TAB. XIX.

FIG. I.

A view of the under ſurface of the female.

a. An expoſure, on flaying off the ſkin, of the right electric organ, which conſiſts of white pliant columns, in a cloſe and for the moſt part hexagonal arrangement, giving the general appearance of a honey-comb in miniature. Theſe columns have been ſometimes denominated cylinders ; but, having no interſtices, they are all angular, and chiefly ſix-cornered.

b. The ſkin which covered the organ, ſhewing on its inward ſide an hexagonal net work.

c. The noſtrils in the form of a creſcent.

d. The mouth in a creſcent contrary to that of the noſtrils, furniſhed with ſeveral rows of very ſmall hooked teeth.

e. The

e. The branchial apertures, five on each ſide.
f. The place of the heart.
g. g. g. The place of the two anterior tranſverſe cartilages, which, paſſing one above and the other below the ſpine, ſupport the diaphragm, and uniting towards their extremities, form on either ſide a kind of clavicle and ſcapula.
h. h. The outward margin of the great lateral fin.
i. i. Its inner margin, confining with the electric organ.
k. The articulation of the great lateral fin with the ſcapula.
l. The abdomen.
m. m. m. The place of the poſterior tranſverſe cartilage which is ſingle, united with the ſpine, and ſupports on each ſide the ſmaller lateral fins.
n. n. } The two ſmaller lateral fins.
n. n. }
o. The anus.
p. The fin of the tail.

FIG.

FIG. II.

A view of the upper ſurface of the female.

a. a. An expoſure of the upper part of the right electric organ.
b. The ſkin which covered the organ.
c. The eyes, prominent and looking horizontally outwards, but capable of being occaſionally withdrawn into their ſockets.
d. Two circular apertures communicating with the mouth, and furniſhed each with a membrane, which in air, as well as in water, plays regularly backwards and forwards acroſs the aperture in the office of inſpiration.
e. The place of the right branchia.
f. The two fins of the back.
g. g. The place of the anterior tranſverſe cartilages.

FIG. III.

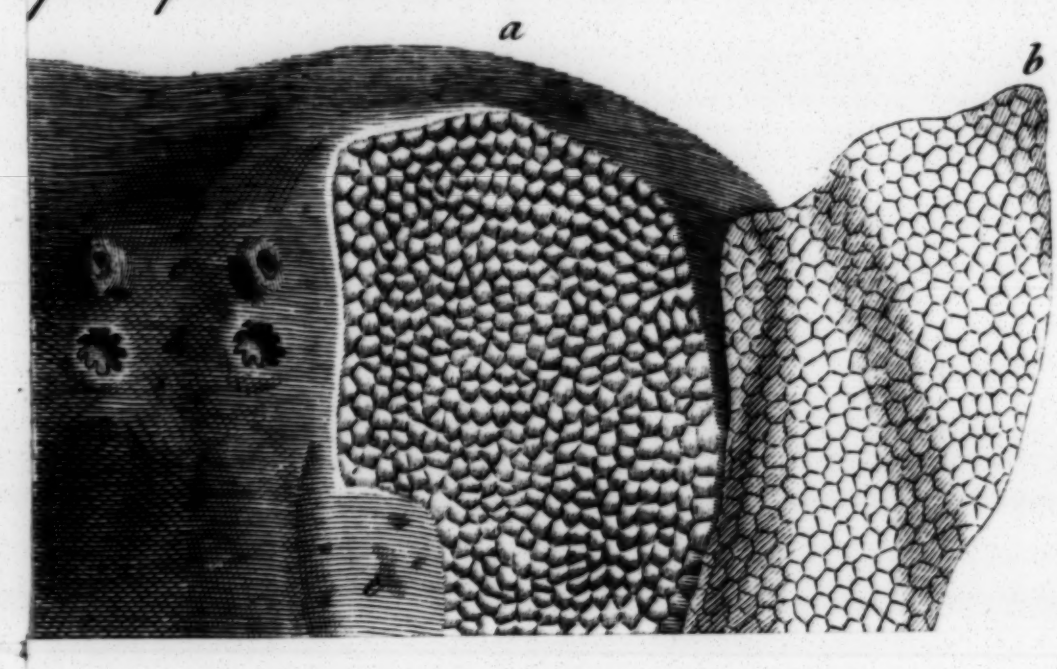
A view of the under ſurface of the male, whoſe ſize, as here repreſented, is, in general, ſmaller than that of the female.

a. a. Two appendices, diſtinguiſhing the male ſpecies.

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Fig. 2.

rface of the Female?.



The Male & Female TORPEDO or

frequenting the Sea Shores in the Neigh

Fig. 1.

Under Surface of the Female.

Fig. 3.

Under Surface of the
which is generally sm
than the Female



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or ELECTRIC RAY,

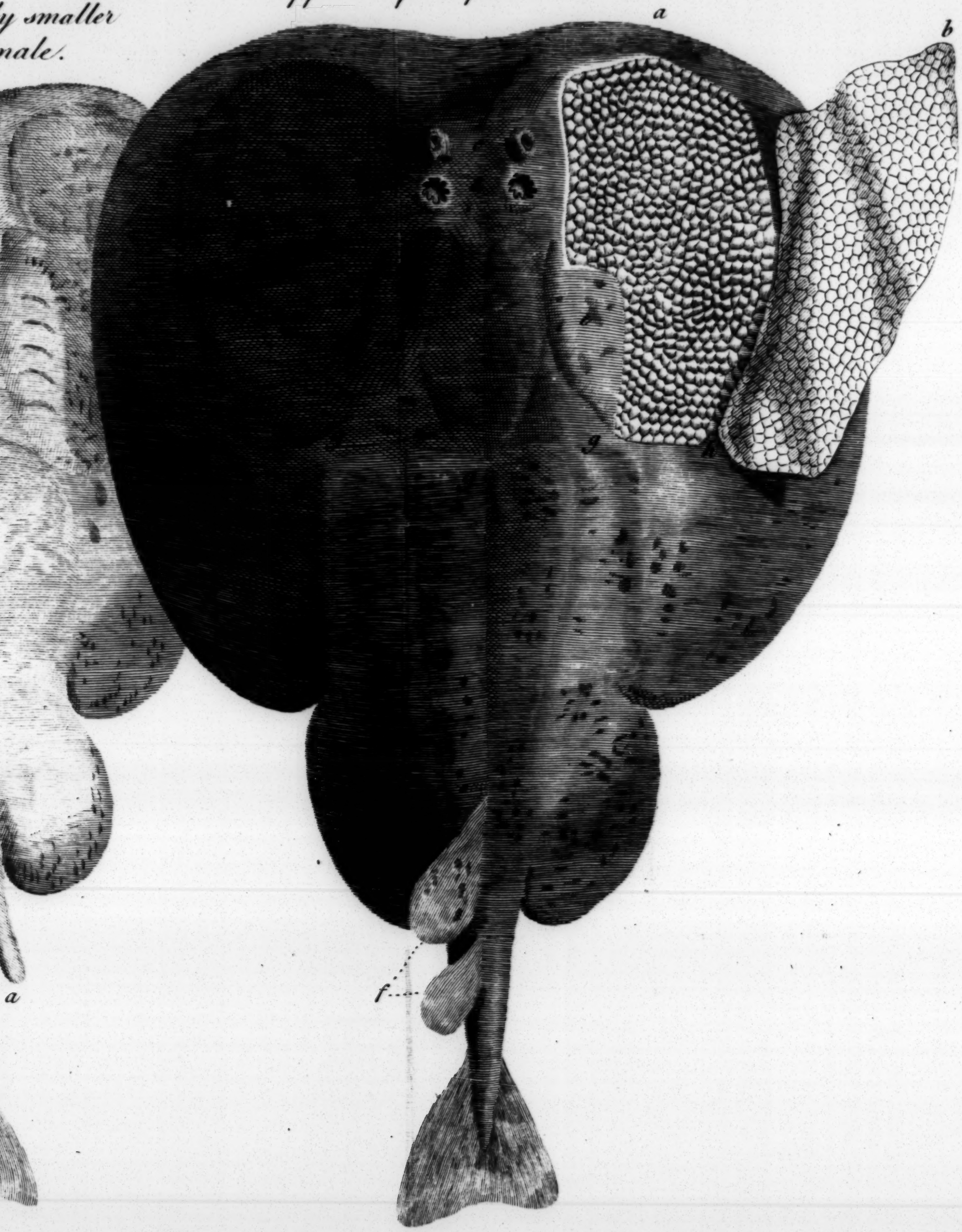
ighbourhood of LA ROCHELLE.

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he Male;
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Fig. 2.

Upper Surface of the Female?



J.s Roberts sculpsit.

XL. *Anatomical Obſervations on the Torpedo.* *By* John Hunter, *F. R. S.*

Read July 1, 1773. I WAS deſired ſome time ſince, by Mr. Walſh, whoſe experiments at La Rochelle had determined the effect of the Torpedo to be electrical, to diſſect and examine the peculiar organs by which that animal produces ſo extraordinary an effect. This I have done in ſeveral ſubjects furniſhed to me by that Gentleman.

I am now deſired by him to lay before the Society, the obſervations I have made; and for the better underſtanding of them, to preſent, on his part, a male and female Torpedo in ſpirits; in the latter of which the electric organs are expoſed in different views and ſections; likewiſe a copper-plate, which he took care to have engraved, exhibiting thoſe organs.

Of the general ſtructure and anatomy of the Torpedo I ſay nothing, ſince the animal does not differ very materially, excepting in it's electric organs (as they have been properly named by Mr. Walſh) from the reſt of the Rays, of which family it is well known to be. I will only premiſe, that the Torpedo, of which I treat, is about eighteen inches long, twelve broad, and in it's central or thickeſt part two inches thick; which is nearly the ſize of

the female ſpecimen, now preſented to the Society, as well as of that from which the plate was taken: but where there is any difference in the organ ariſing from difference in ſize, notice will be taken of it in this account.

The electric organs of the Torpedo are placed on each ſide of the *cranium* and gills, reaching from thence to the ſemicircular cartilages of each great fin, and extending longitudinally from the anterior extremity of the animal to the tranſverſe cartilage, which divides the thorax from the abdomen; and within theſe limits they occupy the whole ſpace between the ſkin of the upper and of the under ſurfaces: they are thickeſt at the edges near the center of the fiſh, and become gradually thinner towards the extremities. Each electric organ, at it's inner longitudinal edge, is unequally hollowed; being exactly fitted to the irregular projections of the *cranium* and gills. The outer longitudinal edge is a convex elliptic curve. The anterior extremity of each organ, makes the ſection of a ſmall circle; and the poſterior extremity makes nearly a right angle with the inner edge. Each organ is attached to the ſurrounding parts by a cloſe cellular membrane, and alſo by ſhort and ſtrong tendinous fibres, which paſs directly acroſs, from it's outer edge, to the ſemicircular cartilages.

They are covered, above and below, by the common ſkin of the animal; under which there is a thin *faſcia* ſpread over the whole organ. This is compoſed of fibres, which run longitudinally, or in the direction of the body of the animal: theſe fibres appear to be perforated in innumerable places; which

gives the *faſcia* the appearance of being faſciculated; it's edges all around, are cloſely connected to the ſkin, and at laſt appear to be loſt, or to degenerate into the common cellular membrane of the ſkin.

Immediately under this, is another membrane, exactly of the ſame kind, the fibres of which in ſome meaſure decuſſate thoſe of the former, paſſing from the middle line of the body outwards and backwards. The inner edge of this, is loſt with the firſt deſcribed; the anterior, outer, and poſterior edges are partly attached to the ſemicircular cartilages, and partly loſt in the common cellular membrane.

This inner *faſcia* appears to be continued into the electric organ, by ſo many proceſſes, and thereby makes the membranous ſides or ſheaths of the columns, which are preſently to be deſcribed; and between theſe proceſſes the *faſcia* covers the end of each column, making the outermoſt or firſt partition.

Each organ, of the fiſh under conſideration, is about five inches in length, and at the anterior end three in breadth, though it is but little more than half as broad at the poſterior extremity.

Each conſiſts wholly of perpendicular columns, reaching from the upper to the under ſurface of the body, and varying in their lengths, according to the thickneſs of the parts of the body where they are placed; the longeſt column being about an inch and an half, the ſhorteſt about one fourth of an inch in length, and their diameters about two tenths of an inch.

The figures of the columns are very irregular, varying according to ſituation and other circumſtances. The greateſt number of them are either irregular Hexagons, or irregular Pentagons; but from the irregularity of ſome of them, it happens that a pretty regular quadrangular column is ſometimes formed. Thoſe of the exterior row are either quadrangular or hexagonal; having one ſide external, two lateral, and either one or two internal. In the ſecond row they are moſtly pentagons.

Their coats are very thin, and ſeem tranſparent, cloſely connected with each other, having a kind of looſe network of tendinous fibres, paſſing tranſverſely and obliquely between the columns, and uniting them more firmly together. Theſe are moſtly obſervable where the large trunks of the nerves paſs. The columns are alſo attached by ſtrong inelaſtic fibres, paſſing directly from the one to the other.

The number of columns in different Torpedos of the ſize of that now offered to the Society, appeared to be about 470 in each organ, but the number varies according to the ſize of the fiſh *. Theſe columns increaſe, not only in ſize, but in number, during the growth of the animal: new ones forming perhaps every year on the exterior edges, as there they are much the ſmalleſt. This proceſs may be ſimilar to the formation of new teeth, in the human jaw, as it increaſes.

* In a very large Torpedo, the number of columns in one electric organ were 1182.

Each column is divided by horizontal partitions, placed over each other, at very ſmall diſtances, and forming numerous interſtices, which appear to contain a fluid. Theſe partitions conſiſt of a very thin membrane, conſiderably tranſparent. Their edges appear to be attached to one another, and the whole is attached by a fine cellular membrane to the inſide of the columns. They are not totally detached from one another: I have found them adhering, at different places, by blood-veſſels paſſing from one to another.

The number of partitions contained in a column of one inch in length, of a Torpedo which had been preſerved in proof ſpirit, appeared upon a careful examination to be one hundred and fifty: and this number in a given length of column appears to be common to all ſizes in the ſame ſtate of humidity, for by drying they may be greatly altered; whence it appears probable that the increaſe in the length of a column, during the growth of the animal, does not enlarge the diſtance between each partition in proportion to that growth; but that new partitions are formed, and added to the extremity of the column from the *faſcia*.

The partitions are very vaſcular; the arteries are branches from the veins of the gills, which convey the blood that has received the influence of reſpiration. They paſs along with the nerves to the electric organ, and enter with them; then they ramify, in every direction, into innumerable ſmall branches upon the ſides of the columns, ſending in from the circumference all around upon each partition ſmall arteries,

arteries, which ramify and anaſtomoſe upon it; and paſſing alſo from one partition to another, anaſtomoſe with the veſſels of the adjacent partitions.

The veins of the electric organ paſs out, cloſe to the nerves, and run between the gills, to the auricle of the heart.

The nerves inſerted into each electric organ, ariſe by three very large trunks from the lateral and poſterior part of the brain. The firſt of theſe, in it's paſſage outwards, turns round a cartilage of the *cranium*, and ſends a few branches to the firſt gill, and to the anterior part of the head, and then paſſes into the organ towards its anterior extremity. The ſecond trunk enters the gills between the firſt and ſecond openings, and, after furniſhing it with ſmall branches, paſſes into the organ near it's middle. The third trunk, after leaving the ſkull, divides itſelf into two branches, which paſs to the electric organ through the gills; one between the ſecond and third openings, the other between the third and fourth, giving ſmall branches to the gill itſelf. Theſe nerves having entered the organs, ramify in every direction, between the columns, and ſend in ſmall branches upon each partition where they are loſt.

The magnitude and the number of the nerves beſtowed on theſe organs, in proportion to their ſize, muſt on reflection appear as extraordinary as the phænomena they afford. Nerves are given to parts either for ſenſation or action. Now if we except the more important ſenſes of ſeeing, hearing, ſmelling, and taſting, which do not belong to the electric organs, there is no part even of the moſt perfect animal, which, in proportion to it's ſize, is

ſo

ſo liberally ſupplied with nerves; nor do the nerves ſeem neceſſary for any ſenſation which can be ſuppoſed to belong to the electric organs. And with reſpect to action, there is no part of any animal, with which I am acquainted, however ſtrong and conſtant it's natural actions may be, which has ſo great a proportion of nerves.

If it be then probable, that thoſe nerves are not neceſſary for the purpoſes of ſenſation, or action, may we not conclude that they are ſubſervient to the formation, collection, or management of the electric fluid; eſpecially as it appears evident, from Mr. Walſh's experiments, that the will of the animal does abſolutely controul the electric powers of it's body; which muſt depend on the energy of the nerves.

How far this may be connected with the power of the nerves in general, or how far it may lead to an explanation of their operations, time and future diſcoveries alone can fully determine.

An EXPLANATION of the Engraving of the TORPEDO.

TAB. XX.

Fig. I. The upper ſurface of the electric organ.

AA. The common ſkin of the animal.

B. The inſpiratory opening.

C. The eye.

D. The part in which the gills are incloſed.

EEE. The ſkin diſſected off from the electric organ, and turned outwards; the honeycomb appearance on it's internal ſurface correſponding with the upper ſurface of the organ.

F. The part of the ſkin which covered the gills, with ſome ramifications of an excretory duct upon it.

GGG. The upper ſurface of the electric organ, formed by the upper extremities of the perpendicular columns.

Fig. II. The right electric organ divided horizontally into nearly two equal parts at the place where the nerves enter; the upper half being turned outwards.

AA. BB. CC. DD. The correſponding parts of the trunks of the nerves, as they emerge from the gills, and ramify in the electric organ.

AA. The firſt or anterior trunk ariſing juſt before the gills.

BB. The ſecond or middle trunk ariſing behind the firſt gill.

Fig. 1.



Fig. 2.

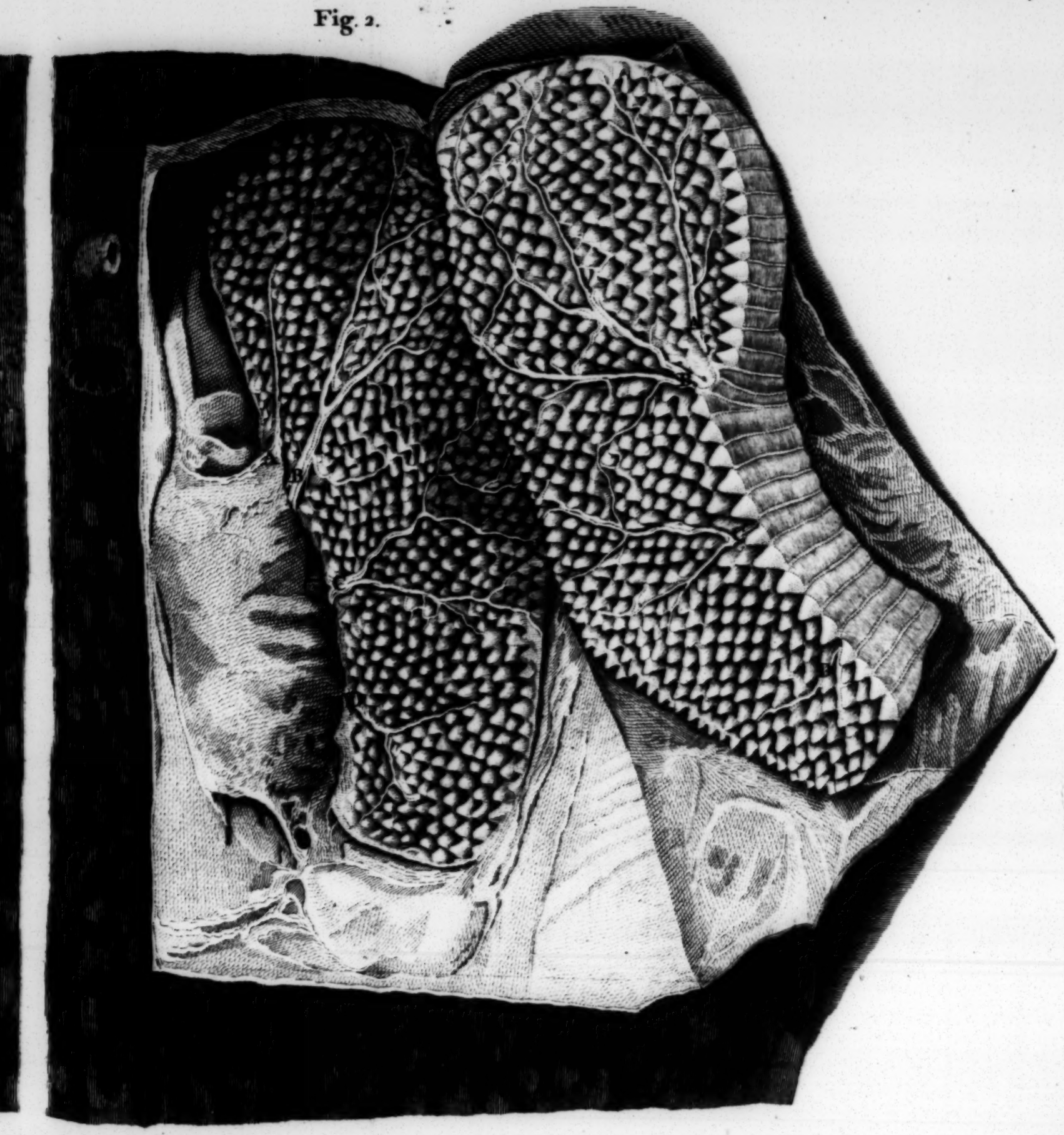
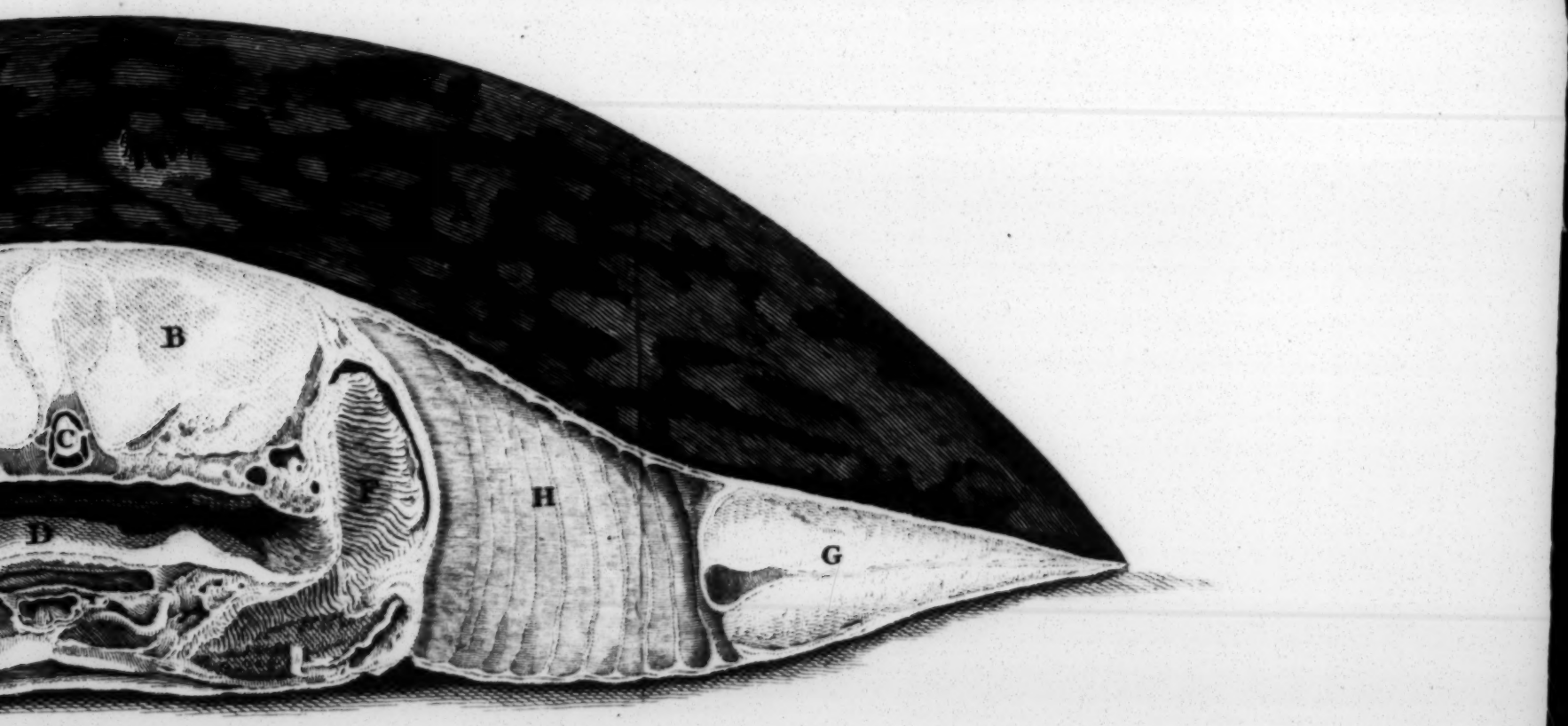
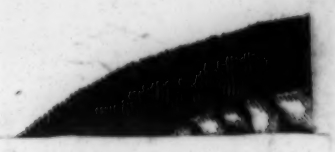


Fig. 3.



J.s Roberts del. et sculp.





CC. The anterior branch of the third trunk arising behind the ſecond gill.

DD. The poſterior branch of the third trunk arising behidd the third gill.

Fig. III. A perpendicular ſection of the Torpedo a little below its inſpiratory openings.

AA. The upper ſurface of the fiſh.

BB. The muſcles of the back as divided by the ſection.

C. The Medulla Spinalis.

D. The Oeſophagus.

E. The left gill ſplit, to expoſe the courſe of a trunk of the nerve through it.

F. The breathing ſurface of the right gill.

GG. The fins.

HH. The perpendicular columns which compoſe the electric organ, with a repreſentation of their horizontal partitions.

I. One of the trunks of the nerves, with its ramifications.

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MDCCLXXIV.

GENTLEMEN,

IT is with great ſatisfaction I enter upon this part of my office—to confer, in your name, the prize-medal of the preſent year upon a Member of this Society ſo worthy of that diſtinction.

The object which Sir GODFREY COPLEY, founder of the benefaction, had in view, and the manner in which the original pecuniary reward was converted into this more liberal form, having been ſo lately explained by my

honoured predeceſſor; I need only obſerve, that though your Preſident and Council have been entruſted with the ſole power of adjudging this premium, yet they have now, as, I am perſuaded, they have had on former occaſions, the greateſt ſolicitude to nominate that perſon, who in their opinion would have obtained all your ſuffrages.

In confidence of ſuch unanimity, it is with ſingular pleaſure I acquaint you, that the Reverend JOSEPH PRIESTLEY, Doctor of Laws, has been found at this time the beſt entitled to ſo public a mark of your approbation, on account of the many curious and uſeful experiments contained in his *Obſervations on different kinds of Air*, read at the Society in March 1772, and inſerted in the laſt complete volume of your Tranſactions [*a*]. And indeed, GENTLEMEN, when you reflect on the zeal which our worthy brother has ſhewn to ſerve the public and to do credit to your inſtitution, by his numerous, learned, and valuable communications, you will, I imagine, be inclined to think, that we have been rather ſlow than precipitate in acknowledging ſo much merit.

[*a*] Vol. 62.

Your

Your time will not allow me to touch on the ſubjects of his former Papers [*b*]; nay I apprehend I ſhall even treſpaſs upon it, by recalling to your memory only a few of thoſe intereſting diſcoveries which Doctor PRIESTLEY has made in theſe *Obſervations:* ſince in doing juſtice to others, as well as to him, it will be proper to remind you of the progreſs that had already been made in this part of ſcience by men of the greateſt abilities in their time, and by other ingenious perſons ſtill among us.

THERE is not perhaps any branch of Natural Philoſophy that has more engaged the attention of the learned, or been more ſucceſsfully cultivated, than the nature of the common air. The knowledge how indiſpenſable it is to the preſervation of animals, muſt have been coeval with mankind: it was from the beginning, as now, *the breath of life.* It was found likewiſe to be a neceſſary ſupport of fire, and that the vegetable creation, deprived of it, languiſhed and died. Nor did the ancient phyſicians fail to diſtinguiſh, at leaſt attempt to

[*b*] In Phil. Tranſ. vol. 58, 59, 60.

diſtinguiſh, between the effects of an air too hot and one too cold, an air too moiſt and one too dry, and between an inſalutary and a wholeſome air.—Thus far the experience, or the theory of all ages.—But the leſs obvious properties of this element, its gravitation and its elaſticity, with their long train of conſequences, remained unknown, till, about the beginning of the laſt century, Lord BACON and GALILEO, in that dawn of philoſophy which they themſelves diffuſed, began the inquiry. The former from experiments aſcertained the elaſticity of the air, and upon that principle conſtructed his *vitrum calendare*, the firſt thermometer [*c*]. The latter diſcovered that air had weight: but though that ornament of Italy was not ignorant of the limited ſuction of a pump, yet to account for the riſe of the water ſo far in it, he ſtill had recourſe to Nature's *abhorrence of a void* [*d*].

TORRICELLI, at laſt, the diſciple of GALILEO, by one happy and deciſive experiment diſcovered the preſſure of the atmoſphere; and PASCAL obſerved, that this preſſure varied according to the heights he carried his barome-

[*c*] Bac. Nov. Org. lib. 2. aph. 13.
[*d*] Dialog. 1.

ter

ter [*e*]. Soon after followed the air-pump, the invention of the celebrated OTTO DE GUERICK; which though at firſt a rude and imperfect inſtrument, yet improved by himſelf [*f*], and more by Mr. BOYLE and Dr. HOOK (two of the illuſtrious fathers of this Society) it ſoon became in the hands of Mr. BOYLE the means of opening the richeſt mines of natural knowledge. In this reſearch, the Hiſtory of the common air, he ſeemed ſo far to carry his inquiries, as to leave little to be done by others who ſhould come after him; thoſe parts excepted depending on geometry and calculation [*g*]. How ſucceſsfully theſe were executed by Dr. HALLEY and Sir ISAAC NEWTON, I ſcarce need mention; nor the ſolid foundation on which thoſe great men eſtabliſhed the rarefaction of the air; and in what proportion, according to its diſtance from the earth [*b*]. But it was

[*e*] Traité ſur l'Equilibre des Liq.

[*f*] Gaſpar. Schott. De Arte Mechan. Hydr. Pneumat. Exp. nova Magdeburg.

[*g*] Boyle, Phyſico-mechan. Exp. & Mem. for a Gen. Hiſt. of the Air.

[*b*] Phil. Tranſ. N° 181. p. 104. Abrid. vol. 2. p. 14. Phil. Nat. Princ. Math. lib. 2. prop. 22, 23.

Sir

Sir Isaac Newton alone, who, upon the principle of the air's being compreſſed by the power of gravity, and that of its elaſticity, taught that tremulous bodies would communicate their motion to the air, and thereby excite vibrations in it, ſpreading every where. Thus he diſcovered the efficient cauſe of ſounds [*i*].

But before this period Mr. Boyle obſerving, as he himſelf informs us, how much air was concerned in many of the *phænomena* of Nature, and how neceſſary it was to the exiſtence of animals, became ſolicitous to inquire, whether a fluid of ſo great importance were not produceable by art; if ſo, he believed that ſuch air might be ſerviceable in life, particularly in the art of diving, and in *ſubmarine navigation* [*k*]. With theſe views that admirable Naturaliſt ſet about making ſome new experiments, and from a variety of bodies, by different proceſſes, obtained a pneumatical fluid (from ripe fruit, fermenting and effervеſcing liquors, and from the putrefaction

[*i*] Phil. Nat. Princ. Mat. lib. 2. prop. 43.

[*k*] An attempt of Cornelius Drebell to make a veſſel to row under water with men in it. See Boyle's works, vol. 1. p. 69. vol. 3. p. 174.

of

of animal and vegetable ſubſtances) anſwering, till then, his only criterion of air, in being of a durably elaſtic nature [*l*]. Yet after all, Mr. Boyle found that theſe new productions were eſſentially different from common air, as they preſently extinguiſhed flame, and ſuffocated thoſe animals that attempted to breathe in them. But though he miſſed finding what he ſo much wanted, his labour was not in vain; philoſophy was enriched with the knowledge of what he called *factitious* or *artificial* air, which has in the end proved as uſeful as he could have wiſhed, in explaining ſeveral natural appearances, and in being ſubſervient to the wants of man.

But this diſcovery, however intereſting to the Naturaliſt, to the Chemiſt in particular [*m*], ſeems to have been little attended to, till in the beginning of this century Sir Isaac Newton obſerved, that true permanent air ariſes from fixed bodies by heat and fermentation; and that thoſe aërial particles recede from one another with the greateſt force, which upon contact cohered moſt ſtrongly:—and that denſe bodies

[*l*] Boyle's Works, vol. 4. p. 236, & ſeq.
[*m*] Hales, Stat. Eſſ. vol. 1. ch. 6. p. 317.

by

by fermentation rarefy into ſeveral ſorts of air; and that this air by fermentation, and ſometimes without it, returns into denſe bodies [*n*]. Excited by ſuch authority, the Reverend Doctor HALES (whoſe amiable as well as philoſophic qualities are ſtill freſh on the minds of ſeveral gentlemen preſent) reſuming thoſe experiments concerning the ſeparation of air from bodies, confirmed and extended the diſcoveries of Mr. BOYLE; ſhewing not only that air entered into the compoſition of moſt bodies, but the very proportion it bore to the reſt of the compound, and that often to an amazing quantity [*o*]. Dr. HALES likewiſe examined the mineral waters, thoſe of Pyrmont particularly; and finding them abounding with air, to that circumſtance he aſcribed the ſpirit and briſkneſs of thoſe fountains. But that excellent author did not ſeem to apprehend, that in this, as in other inſtances, the air which he produced was not the common air, but, if I may be allowed the expreſſion, the *factitious* air of Nature; as being of the ſame kind with what Mr. BOYLE had extracted

[*n*] Compare Newton's Optics, Quer. 30, 31.
[*o*] Stat. Eſſ. vol. 1. ch. 6.

from

from fermenting and effervescing liquors; nay the same with the *mephitis* or deadly vapour of the ancients, or the *mofeta* of the modern Italians, so frequently met with in the caverns, springs, and lakes of their country: and the same with the *stith* or *choak-damp* in our coal-pits, so often fatal to the miners. It must be owned it was hard to conceive, how these springs should owe their prime virtues to what, in another manner of application, Dr. Hales saw was so destructive of vitality.

Now this notion concerning the impregnation of the mineral waters by the *mephitis*, was, as far as I know, originally suggested by a foreign Member, Dr. Seip of Pyrmont, first in a treatise he published in the German language, and afterwards in a communication to this Society, in the year 1736, in which he describes a small cavern at Pyrmont, similar to the *grotta de' cani*, near Naples [*p*]. But when this ingenious author calls that *mephitis* (which is a durably elastic fluid *sui generis*) a *sulphureous steam*, or a *sulphureo-spirituous vapour*, he appears to have been imper-

[*p*] Phil. Trans. N° 448. Abridg. vol. 8. p. 659.

fectly acquainted with its nature; which is now found to conſiſt of nothing inflammable or ſulphureous, and to be of a denſity or ſpecific gravity conſiderably greater than that of common air.

Thus the fuller diſcovery of this principle we owe to Dr. BROWNRIGG of White-haven, who, about thirty years ago, began clearly to unfold this myſtery. But thoſe curious papers were not then inſerted in the Tranſactions, as the too modeſt author had requeſted a delay, till he ſhould be able to make them more worthy of that honour. In that communication he remarks, "That a more intimate acquaintance with thoſe noxious airs in mines, called *damps*, might lead to the diſcovery of that ſubtile principle of mineral waters, known by the name of their *ſpirit*; that the mephitic exhalations, termed the *choak-damp*, he had found to be a fluid permanently elaſtic; and from various experiments he had reaſon to conclude that it entered the compoſition of the waters of Pyrmont, Spa, and others; imparting to them that pungent taſte, from which they were denominated *acidulæ*, and likewiſe that volatile principle,

principle, on which their virtues chiefly depend [q]."

In order to aſcertain a fact of ſo much conſequence, Dr. BROWNRIGG took the opportunity, when at Spa ſeveral years after, to make ſome experiments for this purpoſe; when he had the ſatisfaction to find thoſe waters pregnant with the *artificial* or *factitious* air of Mr. BOYLE, the ſame with that of the ſuffocating *grotta* near Naples, and the ſame with the *choak-damp* of our coal mines; for as much as this air inſtantly extinguiſhed flame, and the life of thoſe animals he had incloſed in it [r]. The ſucceſs of this worthy member, in thus far analyzing thoſe waters, encouraged others to purſue the inquiry; and to inveſtigate the manner in which Nature alſo furniſhed them with the chalybeate principle [s]. Mr. LANE therefore, in conſequence of a converſation with Dr. WATSON junior (both of this Society) upon an experiment of Mr. CAVENDISH's, by which that gentleman had found the mephitic air (ſuch as Dr. BROWNRIGG had detected in Spa-water)

[q] Vid. Phil. Tranſ. vol. 55. p. 236. & ſeq.
[r] Ib. p. 218. & ſeq.
[s] More properly, the *iron-principle*.

ſufficient to diſſolve any calcarious earth [*t*]: in conſequence, I ſay, of this converſation, wherein it was ſurmiſed, that the ſame mephitic air might likewiſe diſſolve iron in common water, he made the experiment with air taken from Spa water, and happily ſucceeded [*u*]. By this means the nature of the metallic principle in mineral waters was clearly explained; and the whole analyſis of thoſe celebrated fountains, ſo often attempted by Chemiſts and others, and ſtill eluding their laboured reſearches, was thus in the moſt ſimple manner brought to light.

Nothing now ſeemed to be wanting to the triumph of Art, but an eaſy manner of joining, as there ſhould be occaſion, one or both of theſe principles to common water; in order to improve upon Nature, in the more extenſive uſe of her medicine. This was effected by Dr. PRIESTLEY, after ſome other important diſcoveries had been made in this part of pneumatics, firſt by Dr. BLACK, Profeſſor of Chemiſtry at Edinburgh, and then by Mr.

[*t*] Phil. Tranſ. vol. 57. p. 92. & ſeq.
[*u*] Ib. vol. 59. p. 216. & ſeq.

CAVEN-

CAVENDISH of this Society. The former has ſhewn that a particular ſpecies of factitious air (he calls it *fixed)* adheres to all calcarious earths, magneſia, and alcaline ſalts, with different degrees of force; and that this fluid can be ſeparated from theſe ſubſtances, and combined again with them, in the ſame manner as an acid. Upon this diſcovery he explained in a clear and ſimple manner many appearances in Chemiſtry, till then deemed the moſt unaccountable. Such was the efferveſcence of abſorbent earths and alkaline ſalts with acids, and the change of the mild calcarious earths into quick lime by heat (in conſequence of the expulſion of this *fixed* air which neutralizes them) [*w*]. I muſt add, that I have been well informed, that for ſeveral years paſt the learned Profeſſor has taught, that the air which unites with alkaline ſubſtances is of the ſame nature with the *mephitis*, or ſuffocating air of the *grotta de' cani* and mines; the ſame with what is emitted from vegetables in fermentation; and that in ſome reſpects it agrees with the air which

[*w*] Eſſ. and Obſerv. Phyſ. & Liter. vol. 2. p. 157. & ſeq.

has

has been injured by the breath of animals, or by the burning of fuel: and laſtly, that the air or elaſtic fluid ariſing from the ſolution of metals by acids is very different from the former.

Mr. CAVENDISH has made ſeveral valuable additions to theſe diſcoveries, not only with regard to that ſpecies of *factitious* air the Profeſſor had denominated *fixed air*, but to other elaſtic fluids. He has with accuracy aſcertained the ſpecific gravity of this fixed air, as expelled from alkaline ſubſtances by acids, or from vegetable matter by fermentation; and has demonſtrated the ſimilarity of airs produced by either of theſe two ways. He has confirmed Dr. BLACK's account of the quantity of the fixed air contained in alkaline ſalts and in alkaline earths. He has ſhewn that this fluid can be mixed with water, and in what proportion; and that it flies off again from the water upon heating it, or expoſing it to the common air. Laſtly, that this ſpecies of factitious air imparts to the water the power of diſſolving abſorbent earths; the experiment, as I obſerved before, which led to the knowledge,

ledge, how Nature infuſed the metallic principle into what are commonly called the *chalybeate waters* [*x*].

Of all theſe facts Dr. PRIESTLEY has carefully availed himſelf. For having learned from Dr. BLACK that this fixed or mephitic air could in great abundance be procured from chalk, by means of diluted ſpirit of vitriol [*y*]; from Dr. MACBRIDE, that this fluid was of a conſiderable antiſeptic nature [*z*]; from Mr. CAVENDISH, that it could in a large quantity be abſorbed by water [*a*]; and from Dr. BROWNRIGG, that it was this very air which gave the briſkneſs and chief virtues to the Spa and Pyrmont waters [*b*]: Dr. PRIESTLEY, I ſay, ſo well inſtructed, conceived that common water impregnated with this fluid alone might be uſeful in medicine, particularly for Sailors on long voyages, for curing or preventing the Sea-ſcurvy. This, we know, is a putrid diſtemper requiring all the antiſeptic quality

[*x*] Phil. Tranſ. vol. 56. p. 141. & ſeq.
[*y*] Eſſ. and Obſerv. Phyſ. & Liter. loc. cit.
[*z*] Experim. Eſſ. *paſſim*.
[*a*] Phil. Tranſ. vol. 56. p. 161. & ſeq.
[*b*] Phil. Tranſ. vol. 55. p. 218. & ſeq.

of

of thoſe mineral waters, without the chalybeate principle, which might injure by overheating the blood, too much diſpoſed to inflammation. For this purpoſe he made a ſimple apparatus for generating this ſpecies of air from chalk and mixing it with water, in ſuch quantities, and in ſo ſpeedy a manner, that having exhibited the experiment before this Society and the College of Phyſicians, it met with ſo much approbation, that, in order the Public might the ſooner reap the benefit of it, he was induced to detach this part of his labours, and in a ſeparate Paper to preſent it to the Admiralty [*c*].

The reſt of his obſervations upon the different kinds of air, addreſſed to the Society [*d*], contain ſo much matter, that I will not preſume to encroach ſo far on your time, as to offer even a ſhort abſtract of the whole; but ſhall be ſatisfied to ſingle out a few of thoſe many diſcoveries, ſuch as are the moſt ſtriking, either for their immediate uſe in life, like that above; or for the explanation

[*c*] A pamphlet intituled Directions for impregnating water, &c.

[*d*] Phil. Tranſ. Vol. 62.

of

of ſome of the more intereſting appearances in Nature.

I come therefore to another ſpecies of *factitious* air, called the *inflammable*. Till within theſe few years little more was known, than that this kind of ſubtile fluid was found in mines, in neglected privies and common ſewers; but chiefly in coal-pits, where it is called the *fire-damp*, making ſometimes formidable exploſions, and indeed often fatal to the miners. I do not recollect that Mr. BOYLE has taken any other notice of it [*e*]. But about 40 years ago Sir JAMES LOWTHER Baronet, favoured the Society with an account ſomewhat more particular of this production of his coal-mines in Cumberland, accompanying it with ſeveral bladders filled with that fluid, which in this houſe burnt as readily, as at its ſource a month before. Yet ſtill this extraordinary ſubſtance was conſidered more as an object of curioſity, than as one of philoſophical inquiry, till Mr. CAVENDISH began to make experiments upon

[*e*] Boyle's Works, vol. 3. p. 101. vol. 5. p. 305, 306.

it; by which, and the conſequences drawn from them, he has added another conſiderable branch to the doctrine of aërial fluids.

Firſt, he has taught how to produce at will, and in great abundance, this other permanently elaſtic fluid from three metallic bodies, Zinc, Iron, and Tin, by diſſolving them in the diluted vitriolic acid, or ſpirit of ſea-ſalt. This ſpecies of factitious air he has ſhewn to be ſurprizingly light, being no more than the tenth part of the weight of common air, and therefore totally different from the *mephitis*, that other ſpecies of factitious air we have been treating of, and which, as was obſerved, is heavier than the air of our atmoſphere. Laſtly, Mr. CAVENDISH has given ſeveral experiments upon the inflammability of various mixtures of this fluid with common air, which are likewiſe new; and, like the reſt, have been made with great preciſion.

Now, though Dr. PRIESTLEY has alſo improved upon this inquiry, by the addition of a variety of experiments; in particular, by ſhewing how this air becomes miſcible with water, and deprived of its inflammability; by comparing

paring it with other ſpecies of factitious air, in regard to conducting the electrical fluid; by inquiring how far it may be conſidered as common air loaded with the principle of fire, called *phlogiſton* by the modern Chemiſts; with other curious obſervations on this ſubſtance: yet all theſe, with other kinds of factitious air, as I have already too long detained you, I muſt with regret paſs over; one other ſpecies excepted, as I reckon it among the moſt brilliant of Dr. PRIESTLEY's diſcoveries [*f*].

This ſpecies he calls the *nitrous air*, without inſiſting on the propriety of the expreſſion. It was firſt produced from the Walton pyrites by means of the ſpirit of nitre. Dr. HALES, who made the experiment, obſerved, that when joined to common air, an effer-

[*f*] I might have added another new ſpecies of factitious air, which he terms *acid*, firſt taken notice of by Mr. CAVENDISH, and more fully inveſtigated by Dr. PRIESTLEY. This is an elaſtic vapour expelled by heat from ſpirit of ſalt, and not liable afterwards to be condenſed by cold. Water readily imbibes this air, and by that means becomes a ſtrong ſpirit of ſalt. The ſame acid air or vapour, he has alſo diſcovered to be a decompoſer of ſubſtances that contain *phlogiſton*, and with them to form a proper inflammable air.

veſcence enſued, with a turbid red colour of the mixture, and an abſorption of part of the common air [*g*]. Dr. PRIESTLEY extending the experiment to other metallic ſubſtances obſerved, that the ſame kind of air was by the ſame acid readily procured from iron, copper, braſs, tin, ſilver, quickſilver, biſmuth, and nihil; and that though it conſtantly, when joined to common air, exhibited thoſe appearances mentioned by Dr. HALES, and more conſpicuouſly in proportion to the purity of the common air mixed with it (that is, its fitneſs for reſpiration); yet it made no change with either fixed or inflammable air, or that air tainted by the breath of animals, or the corruption of their bodies. By means of this teſt he was enabled to judge of the kind, as well as of the degree of injury, done to common air by candles burning in it; and to perceive a real difference in the air of his ſtudy, after a few perſons had been with him there. Nay, a phial of air having been ſent him from the neighbourhood of a large town, it appeared upon a comparative trial

[*g*] Stat. Eſſ. vol. 2. p. 280.

to

to be inferior in quality to that taken up near Leeds, where he then reſided. It was upon ſuch a proſpect of obtaining a criterion for diſtinguiſhing good air from bad, that Lord BACON almoſt in a rapture breaks out: "Theſe are noble experiments that can make this diſcovery; for they ſerve for a natural divination of ſeaſons!" and again, "They teach men to chooſe their dwelling for their better health [*b*]."

Nor is this all the uſe of the nitrous air; Dr. PRIESTLEY ſhews it to be one of the ſtrongeſt antiſeptics. The fixed air has been proved by Dr. MACBRIDE, as was remarked, to be powerful in this particular; but this ſpecies of factitious air has been found to be of ſuperior efficacy. And as our author has diſcovered it to be miſcible with water, he has reaſon to believe it may be applied to various purpoſes, ſuch as the preſervation of the more delicate birds, fiſhes, fruits, and anatomical preparations.

I ſhall now conclude with ſhewing from Dr. PRIESTLEY, what reſources Nature has

[*b*] Nat. Hiſt. Exp. 777.

in ſtore againſt the bad effects of corrupted air, which from various cauſes infect our atmoſphere.

It is well known that flame cannot long ſubſiſt without a renewal of common air. The quantity of that fluid which even a ſmall flame requires is ſurprizing: an ordinary candle *conſumes*, as it is called, about a gallon of air in a minute. Now, conſidering the vaſt conſumption of this vital fluid by fires of all kinds made by man, and by volcanos, it becomes an intereſting inquiry, to aſcertain what change is made in the air by flame; and to diſcover what proviſion there is in Nature, to repair the injury done by this means to our atmoſphere. Dr. PRIESTLEY, after relating the conjectures of others, and not finding them ſatisfactory, was fortunate in falling upon a method of reſtoring air, which had been vitiated by the burning of candles in it. This led the way to the diſcovery of one of the great reſtoratives which Nature employs for this purpoſe; to wit, vegetation: ſee by what induction he proves his opinion.

It

It was natural to imagine, that ſince the change of common air is neceſſary to vegetable, as well as to animal life, both plants and animals rendered it foul in the ſame manner, ſo as to become unfit for further life and vegetation. But when with that expectation the Doctor had put a ſprig of mint, in a growing and vigorous ſtate, under an inverted glaſs-jar ſtanding in water, he was agreeably diſappointed to find, that this plant not only continued to live, though in a languiſhing way, for two months, but that the confined air was ſo little corrupted by what had iſſued from the mint, that it would neither extinguiſh a candle, nor kill a ſmall animal which he conveyed into it. What further evinced the ſalutary nature of the *effluvia* of vegetables; he found, that air vitiated by a candle left in it till it burnt out, was perfectly reſtored to its quality of ſupporting flame, after another ſprig of mint had for ſome time vegetated in it. And to ſhew that the aromatic vapour of that plant had no ſhare in reſtoring this purity to the air, he obſerved, that vegetables of an offenſive ſmell, and even ſuch as ſcarce had any ſmell

at

at all, but were of a quick growth, proved the very beſt for this purpoſe. Nay more, the virtue of growing vegetables was found to be an antidote to the baneful quality of air corrupted by animal reſpiration and putrefaction.

We have ſaid, that neither candles will burn nor animals live beyond a certain time in a given quantity of air; yet the cauſe of either ſo ſpeedy a death or extinction was unknown; nor was any method diſcovered for rendering that empoiſoned air fit again for reſpiration. Some proviſion however there muſt be in Nature for this purpoſe, as well as for that of ſupporting flame: without ſuch the whole atmoſphere would in time become unfit for animal life, and the race of men as well as beaſts would die of a peſtilential diſtemper. Yet we have reaſon to believe, that in our day the air is not leſs proper for breathing in, than it was above two thouſand years ago; that is, as far as we go back in Natural Hiſtory. Now, for this important end, the Doctor has ſuggeſted, to the Divine as well as to the Philoſopher, two grand

reſources

reſources of Nature: the vegetable creation again is one, and the ſea and other great bodies of water are the other.

As to the former, having found that plants wonderfully thrive in putrid air, he began to attempt by means of growing vegetables to purify air that had been injured by animal reſpiration and putrefaction; nor was he leſs ſucceſsful than before. Theſe plants were ſure to recover the air to a degree of fitneſs for breathing in it, and that in proportion to their vigour, and the care he took to remove the rotten leaves and branches; which remaining would have marred the operation.

And with regard to the ſecond reſource of Nature, namely the ocean and other waters, Dr. PRIESTLEY having obſerved, that both the air corrupted by the breath of animals, and that vitiated by other putrid matter, was in a good meaſure ſweetened by the ſeptic part infuſing itſelf into water, he concluded, that the ſea, the great lakes and rivers, which cover ſo large a proportion of the globe, muſt be highly uſeful, by abſorbing what is putrid, for the further purification of the atmoſphere:

thus beſtowing what would be noxious to man and other animals, upon the formation of marine and other aquatic plants, or upon other purpoſes yet unknown.

From theſe diſcoveries we are aſſured, that no vegetable grows in vain, but that from the oak of the foreſt to the graſs of the field, every individual plant is ſerviceable to mankind; if not always diſtinguiſhed by ſome private virtue, yet making a part of the whole which cleanſes and purifies our atmoſphere. In this the fragrant roſe and deadly nightſhade co-operate: nor is the herbage, nor the woods that flouriſh in the moſt remote and unpeopled regions unprofitable to us, nor we to them; conſidering how conſtantly the winds convey to them our vitiated air, for our relief, and for their nouriſhment. And if ever theſe ſalutary gales riſe to ſtorms and hurricanes, let us ſtill trace and revere the ways of a beneficent Being; who not fortuitouſly but with deſign, not in wrath but in mercy, thus ſhakes the waters and the air together, to bury in the deep thoſe putrid and peſtilential *effluvia*, which the vegetables upon the face

face of the earth had been inſufficient to conſume.

THIS, GENTLEMEN, is what I had to ſay upon the occaſion: perhaps too much; but the fruitfulneſs of the ſubject, with my earneſt deſire of commemorating ſome of the more important experiments and concluſions of Dr. PRIESTLEY, and of thoſe who preceded him in theſe inquiries, will, I hope, plead my excuſe. Nor can I conclude without congratulating this illuſtrious Body on the poſſeſſion of ſo many members and friends, ſo capable to promote the great ends of this inſtitution; and who have within theſe few years ſo eminently diſtinguiſhed themſelves, by the lights they have thrown, not only upon this, but upon other of the more ſubtile fluids of Nature. You will underſtand, that to theſe diſcoveries upon factitious air, I join thoſe amazing ones upon magnetiſm and electricity, with all the uſes reſulting from them. Here you will recollect the prediction of hi[illegible] beſt taught the method of inveſtigating [illegible] ſophical truth, the incomparable Lord b[illegible]

who, with that ſpirit of divination peculiar to exalted genius, aſſured his diſciples, that when men ſhould ceaſe to trifle in framing *hypotheſes*, and building haſty ſyſtems; and ſhould by a proper induction from ſober and ſevere experiments attain to the knowledge of the *forms* of things [their more intimate qualities and laws] they ſhould in the end command Nature, and perform works as much greater than were ſuppoſed practicable by the powers of natural magic, as the real actions of a *Cæſar* ſurpaſſed the fictitious ones of the hero of a romance [*i*]. Some earneſt, nor that inconſiderable, of this magnificent promiſe this Society has already obtained. Let thoſe who doubt, view that Needle, which, untouched by any loadſtone, directs the courſe of the Britiſh mariner round the world; or that apparatus, ſo perfectly imitating the long ſuppoſed inimitable lightning; or that other, which diſarms the clouds of that tremendous meteor: or (not to depart from my ſubject) let them ſee how Art can

[*i*] Compare Bac. De Dignit. et Augment. Scient. lib 3. cap. 5.

from

from chalk only, the leaſt promiſing ſubſtance, generate, call it unfetter a copious elaſtic fluid impriſoned in it, the poiſon of man, or his medicine, according to the mode of application; which, though inviſible, yet diſſolves earth and metals, and imparts the ſpirit and virtue to the moſt prized of mineral waters. Yet theſe are but inventions of yeſterday: I would ſtrictly ſay, inventions within the memory of my youngeſt hearer. If to theſe late acquiſitions, ſo honourable to this Society, I add thoſe in Natural Hiſtory, by the zeal and unwearied attention of ſome worthy members, who have extended your correſpondence and adorned your Muſeum; and by thoſe other gentlemen, who, animated with a noble ſpirit, have, to their laſting honour, undertaken the moſt dangerous and moſt diſtant voyages in purſuit of Natural Knowledge: I ſay, when to the progreſs you are making in Experimental Philoſophy, I add that in the Hiſtory of Nature, every true lover of ſcience will rejoice to think, that your affairs have not, perhaps, at any period, been in a more flouriſhing condition.

Dr.

Dr. PRIESTLEY,

It is now time that, in the name and by the authority of the Royal Society of London, inſtituted for the improvement of Natural Knowledge, I preſent you with this medal, the palm and laurel of this Community; as a faithful and unfading teſtimonial of their regard, and of the juſt ſenſe they have of your merit, and of the perſevering induſtry with which you have promoted the views, and thereby the honour of this Society. And in their behalf I muſt earneſtly requeſt you, to continue thoſe liberal and valuable inquiries, whether by further proſecuting this ſubject, probably not yet exhauſted, or by inveſtigating the nature of ſome other of the ſubtile fluids of the univerſe. You will remember, that *Fire*, the great inſtrument of the Chemiſts, is but little known even to themſelves; and that it remains a *Query*, what was by the moſt celebrated of philoſophers propoſed as ſuch, whether there be not a certain fluid (he calls it *Æther*)

the

the cauſe of gravity, the cauſe of the various attractions, and of the animal and vital motions [*k*]. Theſe, Sir, are indeed large demands: but the Royal Society have hitherto been fortunate in their pneumatic reſearches. And were it otherwiſe, they have much to hope from men of your talents and application, and whoſe paſt labours have been crowned with ſo much ſucceſs.

[*k*] Newton's Optics, Quer. 18—24.

ERRATA.

Page.	Line.		
3.	19.	for *fire, and*	r. *fire; and they saw*
12.	5.	for *he*	r. Mr. LANE
20.	8.	for *nihil*	r. *nickel*
29.	2.	for *call*	r. *or call*